SEPTEMBER,
1937

RC
423
.A1
J86

The JOURNAL of SPEECH DISORDERS

Editor

G. OSCAR RUSSELL, PH D.
Columbus, Ohio

Collaborating Editors

JEAN TARNEAUD, M. D.
Paris, France

EMIL FRÖSCHELS, M. D.
Vienna, Austria

M. NADOLECZNY, M. D.
München, Germany

DOUGLAS GUTHRIE, M. D.
Edinburgh, Scotland

YOSHIO HAYASHI, PH. D.
Tokyo, Japan

Official Publication, of the American Speech Correction Association in its 12th year.
Editorial Office: Speech Clinic, Ohio State University, Columbus, Ohio.
Volume 2 : Number 3

$3.00 per Year
Single issue $1.00

CONTENTS

PAGE

Motor Disorders of the Central Nervous System and Their Significance for Speech. Part I. Cerebral and Cerebellar Dysarthrias.....*Paul J. Zentay, M.D.* 131

Vue Synthetique des Principales Acquisitions de Physiologie et de Pathologie Vocales Dues a la Laryngostroboscopie, *Raoul Husson, M.D.* 139

The Preparatory Set in Stuttering.....*C. Van Riper, Ph.D.* 149

Speech Training in the Child Crippled by Spastic Paralysis, *Robert L. Carroll, M.D.* 155

Book Reviews.....*Charles H. Voelker* 158

Program, The American Speech Correction Association, Chicago, Illinois, October 11, 12, 13, 1937..... 159

On the Correction of Foreign Brogue. Breath Stream Control During Transitions in French.....*James L. Barker* 163

Periodical Reviews.....*Charles H. Voelker* 184

The Relation of Bilingualism to Stuttering. A Survey of the East Chicago, Indiana, Schools, *Lee Edward Travis, Ph.D., Wendell Johnson, Ph.D. and Jayne Shover* 185

Bibliography..... 190

The JOURNAL OF SPEECH DISORDERS is published quarterly in March, June, September, and December of each year by the American Speech Correction Association; $1.00 a copy; $3.00 a year (Canada and foreign, $3.50). All communications and subscriptions should be sent to the editorial and business office: Speech Clinic, 324 Derby Hall, Ohio State University, Columbus, Ohio.

Application for entry as second-class matter pending at the Post-Office at Columbus, Ohio.

MOTOR DISORDERS OF THE CENTRAL NERVOUS SYSTEM AND THEIR SIGNIFICANCE FOR SPEECH[1]

PART I. CEREBRAL AND CEREBELLAR DYSARTHRIAS

PAUL J. ZENTAY, M. D.
St. Louis

The purpose of this paper is to discuss the manifold motor disturbances of speech that occur with lesions of the various motor centres and tracts in the central nervous system. These disturbances may conveniently be grouped together under the name of dysarthrias of speech. Their analysis and clear understanding in the light of newer knowledge of the pathophysiology of the central nervous system are extremely important for all who are interested in the study and correction of disorders of speech. Insight gained into these problems must contribute greatly to the evaluation of the potentialties and limitations of speech in a given case.

In the present discussion we shall ignore those disturbances on the highest functional levels that are known under the name of aphasia and apraxia. For practical purposes of this presentation we even have to presuppose that functions on these levels are more or less intact.

Still, nothing appears more suitable as a starting point for our discussion than a quotation from Weisenburg's recent admirable work on aphasia: "Our views on localization, so far as it concerns language, differ from those largely held by the localizationists in that we believe that it is impossible to delimit areas for the language function or any of its processes. The statement of Hughlings Jackson, made almost 70 years ago, that "To locate the damage which destroys speech and to locate speech are two different things,' is just as true today as it was then. It is evident that it is impossible to localize language. In the majority of individuals, language permeates the thought processes to such an extent that the one cannot be separated from the other, and for the present at least it is impossible to give an adequate explanation of intelligence, much less to localize it. That it is the result of the activity of the entire brain, however, there is no doubt."

It is evident that in disorders on this level the most hopeful approach is the psychological. Below the cortical and subcortical level, on the other hand, speech function consists largely of the proper transmission and conduction of motor stimuli and, therefore, the

[1]Read before the American Speech Correction Association, St. Louis, December 30, 1936.

most useful and practical method of study will be the neurophysiological approach. In such a study we necessarily have to bear in mind constantly that in speech as well as in other motor performances we are dealing with functions of the entire central nervous system. It also must be remembered that in any loss of motor function the final results will be dependent just as much on the lesion itself as on the parts intact and capable of compensation or substitution.

The motor impulses of speech originating in the so-called speech centres of the cortex are transmitted to the motor areas of the parietal and frontal lobes. From this point on they follow exactly the same pathways as any other conscious-voluntary motor innervation. These pathways are: 1. Corticobulbar (pyramidal) tracts; 2. corticostriopallidorubrobulbar (extrapyramidal) tracts; 3. frontopontocerebellorubrobulbar tracts.

As evident, all three of these tracts converge on the ultimate efferent lower motor neurons in the bulbar nuclei of the cranial nerves. It goes without saying that speech can be physiological only if the functioning of all three motor systems is intact. They not only have to function together harmoniously and simultaneously, but they maintain mutually a proper tonus and balance. Lesions in any of the three systems lead to well defined disorders and will necessarily have also their concomitant psychological characteristics.

Due to the nature of the lesions usually affecting these motor systems, it is evident that speech disorders as a rule will form only part of a more widespread motor disturbance. It is also easily understandable that speech disorders observed with such lesions will largely bear the same general characteristics as found in other parts of these motor systems. Lesions in any of the three motor systems may be present, of course, without any involvement of the tracts serving the functions of speech. On the other hand, the more extensive the lesions are, the more there is likelihood that speech also will be involved. Such an involvement of speech may present all possible degrees and all forms of individual variations. The descriptions in the further part of this presentation must be looked upon as generalizations and types, not as clinical entities.

Corticobulbar Tract Lesions: These are known also under the name of pyramidal tract lesion, upper motor neuron lesion, or spastic paralysis. They are due to a partial or complete interruption of the pathways at any point between the parietal cortex and the nuclei of the cranial motor nerves. There are no separate pyramidal tracts known for speech. This function is served by the same identical cortical areas and tracts as other motor innervations going to the nuclei of the Vth, VIIth, IXth, XIth and XIIth cranial nerves. Any or all of these tracts may be involved in a pathological process and the resultant disturbance evidently will depend on the extent of the lesions. The closer the destructive process is to the nuclei, the more the picture will resemble that of pseudobulbar paralysis with a more or less complete loss of speech. The tracts to the nuclei of the IXth, XIth and XIIth nerve are particularly important in this respect.

If the pathological lesion is in the subcortical areas or, still more, if it is in the cortex itself, the anatomical conditions make it possible that at times only some of the above tracts are involved, or that the destruction is only partial. In such cases we are dealing with the various forms and degrees of the so-called pyramidal or spastic speech. The type of spastic speech observed is largely determined by the extent of the involvement of the muscles of the speech organs. Great differences will be noticeable if only the facial or the lingual or laryngeal muscles are affected or if all of them are paralyzed. The disturbance will be also decisively influenced by the presence of unilateral or bilateral lesions in the cortex or further below.

On the whole the spastic speech is characterized by the loss of conscious voluntary innervations and increase of muscle tonus and

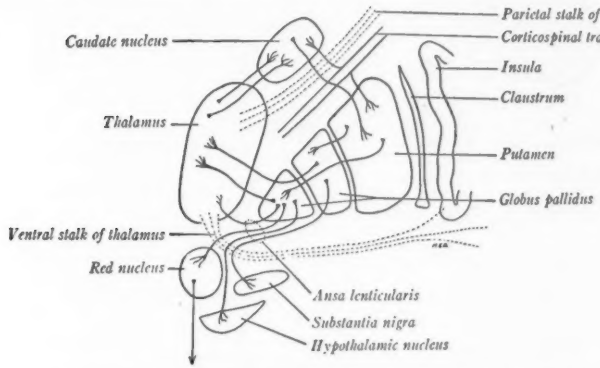


Diagram of the connections of the caudate and lenticular nuclei.

reflex irritability. It is a slow dragging speech with indistinct articulation, apparently requiring a great deal of effort and often accompanied by facial distortions. At times the slowness alternates with explosiveness.

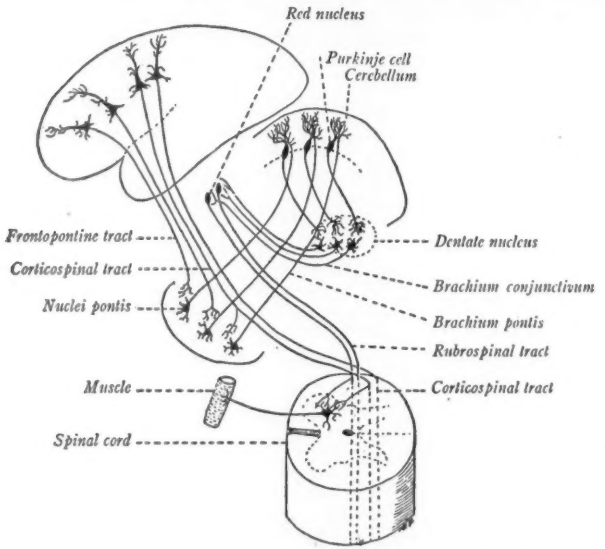
Neurophysiology does not know of any possibility of compensation or substitution for functions lost in the conscious-voluntary pyramidal system; therefore, with such lesions the development of speech depends on the residues.

Corticostriopallidorubrobulbar Tracts (Extrapyramidal Innervation): In analogy with other somatic motor functions the innervations to muscles of the speech organs travel through these tracts simultaneously with those reaching the same muscles through the pyramidal tracts.

In the hierarchy of this system the striate body (caudate nucleus) occupies the highest position. It seems that the caudate nucleus and the subservient extrapyramidal system perform the following func-

tions in the mechanism of speech: (*a*) Regulating and properly distributing the tonus of muscles; (*b*) coordinating and connecting with speech a host of ready and fully developed automatisms; (*c*) regulating the necessary synkineses and essential coinnervations; (*d*) controlling and regulating the pre-existing automatic motor functions, such as maintaining the required tone, rate and depth of respiration, pitch of voice, tempo of speech.

It is evident that all these innervations are vitally important for normal speech. They take place without any conscious effort and



The cortico-ponto-cerebellar and cerebello-rubro-spinal paths.
(Modified from Cajal)

their significance becomes manifest when some kind of pathological process interferes with the functions of this system. The caudate nucleus apparently shows a definite somatotopic distribution of its functions. Lesions, therefore, will lead to various forms of disturbances dependent on their localization. The same is true of lesions affecting other parts of the extrapyramidal system. The common characteristics of these speech disturbances will be that they always appear in the form of dysarthrias and that they usually are associated with other manifestations of so-called extrapyramidal syndromes.

Two large groups of such syndromes are known: (*a*) Hypotonic-hyperkinetic forms, such as athetosis, chorea, torsion spasm; (*b*) hypertonic-hypokinetic forms, such as the Parkinsonian syndrome.

In both of these groups the speech shows very definite characteristics. In the first group we find hyperkinesis of the vocal cords, involuntary movements of the diaphragm, and incoordination of various movements. The result of which is that the voice will be weak and pitch will suffer, leading in extreme cases to a complete disability of speech.

In the second group there is an extreme slowing down of movement, due to difficulty of "breaking through" of innervations. Speech in such cases is hesitant, halting, often interrupted or delayed for many seconds or minutes; it is also lacking in modulation and in advanced cases becomes more and more blurred.

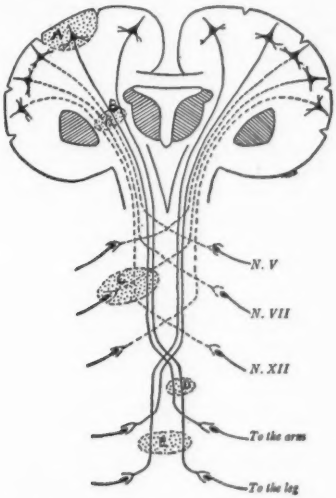


Diagram to illustrate the effects of lesions in various parts of the motor path.

It seems from all the above that the striopallidum occupies an extremely important position in the pathophysiology of speech.

Liepman and Raecke in their hypothesis postulated the existence of some kind of a mechanism interpolated between kinesthetic memory or amnesis and the effective motor apparatus of speech. Lesions of such a mechanism should—according to this theory—lead to dysarthrias of speech. Such an interpolated mechanism is represented unquestionably at the present state of our knowledge by the striate body.

Paul Schilder developed an interesting hypothesis, basing the same on facts gathered from the study of dysarthrias of speech in extrapyramidal lesions. He discovers many points of relations and

similarities between disorders resulting from lesions in the striopallidum or in the mesencephalon and between the problem of stuttering. "Here we may find an important lead for the explanation of the pathophysiology of stuttering. Without having any reason to doubt the psychogenic origin of stuttering, it must be linked to functions of certain parts of the brain. Careful psychologic studies of organic brain lesions will necessarily bring about the desire to explain the neuroses on the basis of brain pathology.

"In this sense we may classify stuttering together with tics as 'striate body neurosis'."

Frontopontocerebellar Tracts: The principal functions of this system are regulating muscle tonus and balance, coordination, controlling the proper measure of motions.

Without doubt a complicated working together of the various muscle groups and apparatus in speech is absolutely necessary. Still the concepts gained from the coordination of body musculature are very inadequate.

Dysarthrias of speech in cerebellar lesions may be described under the following headings:

(*a*) *Ataxic Speech:* It is due to lesions of the coordinating cerebellar tracts for articulation, respiration and phonation. These tracts run through the subthalmic region to the motor centres of phonation localized in the medulla.

Motor incoordination may involve various parts of the complex mechanism of speech in varying degree; *i. e.*, articulation, respiration and phonation may be interfered with in an uneven measure.

(*b*) *Adiadochokinesis:* Here the attained contraction of muscles decreases very rapidly. On the extremities this manifests itself so that repeated movements gradually decrease in intensity and finally cease. In speech similar manifestations may be noted. In spontaneous speaking adiadochokinesis may be overcompensated by slowing down. Aside from this secondary slowness of speech, there is occasionally also a primary slowness with cerebellar lesions.

(*c*) *Explosive-hesitant Speech:* It is apparently due to a disturbance of the inhibiting and braking function of the cerebellum. The loss of this function results in alternating hypo- and hypermetria of muscle innervations.

(*d*) *Scanning Speech:* It consists of stretching of syllables, which are also being sharply cut off from one another.

The site of the lesion is not quite clearly known, but it seems that scanning speech is due to an interruption of the cerebellar tracts at some point between cerebellum, red nucleus and bulbar centres.

Thus the pathophysiology of the three large motor systems in its relationship to speech illustrates clearly the fact that their intactness and harmonious balance is indispensable for normal speech function. Disturbance in any of the partial functions maintained by these systems throws the entire complex mechanism out of gear and although a certain amount of correction and compensation is possible, physiological conditions never can be reestablished in cases where the

damage is irreparable and permanent. Any plan of training for patients with dysarthrias of speech must be conceived with the clear understanding of this situation. Furthermore, the methods applied will have to be carefully adapted to the nature of the motor disturbance that happens to be present.

A few additional theoretical considerations may be helpful in further clarifying the problems before us.

Phylogenetically the pyramidal system is the latest acquisition of the race. Its highly developed and fine performances, such as the use of the hands and of the speech organs, are possible only through the enormous development of the hemispheres of the human brain. It seems further that the frontal lobe is the place where the three

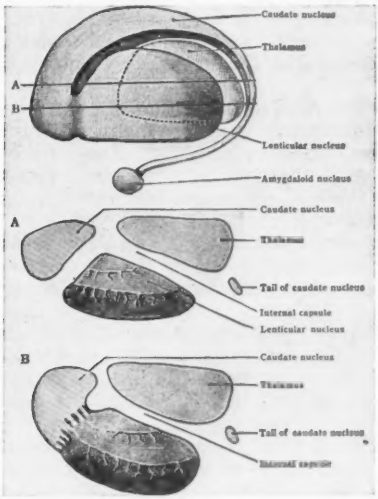


Diagram of lateral view and sections of the nuclei of the corpus striatum with the internal capsule omitted. A and B below represent horizontal sections along lines A and B in the figure above. The figure also shows the relative position of the thalamus and the amygdaloid nucleus. (Jackson-Morris.)

large motor systems are coordinated and brought into higher correlations. It is the frontal lobe, according to the theory of H. Hoff's, that establishes new connections for impulses and takes care of their redistribution. It is here where through repression and regulation of primitive functions the foundation for speech development is created. Finally, it is the frontal lobe from where the connections to the basal ganglia (caudate nucleus) and to the cerebellum lead. Through these connections speech is maintained in a kind of suspension between a conscious and half-conscious state that makes the simultaneous performance of several motor functions possible.

The corticobulbar (pyramidal) system as the developmentally latest part of the motor system is, so to say, superimposed upon the more archaic and more primitive other two systems. This explains why, in case of loss of functions of the pyramidal system, the other two systems may perform fairly completely certain primitive motor functions, while in lesions of these latter systems every kind of pyramidal function will be seriously interfered with.

In other words, the extrapyramidal and cerebellar motor systems may compensate with a fair degree of completeness for the pyramidal system in primitive functions, but not in speech. On the other hand, whenever motor functions of speech served by these two more primitive systems are damaged, there is no possibility of compensation on part of the pyramidal system.

Not only is it impossible for the pyramidal system to compensate for the other two systems, but every normal function in speech of this system is predicated on the undisturbed functioning of the other two systems.

Very much will depend in the process of correction and compensation on residual functions in all three motor systems. More careful study of these must form an essential part of any studies in the future.

The psychological concomitant phenomena also will require a great deal of clarification. At present extremely little is known about this side of the problem.

The practical application of the above discussion to the clinical handling of patients must result in the development of a more comprehensive plan of cooperation between various specialties than in the past.

No patient suffering with dysarthria of speech can be handled intelligently without a detailed neurological examination, the purpose of which must be to determine the nature and extent of the motor disturbance and the outlook for compensatory reeducation based on residual functions.

Psychological studies must determine the abilities and disabilities of the patient and must give an evaluation of the total personality. The methods of these studies still await their development and adaptation to the specific conditions of the problem.

Finally, the physiotherapist and orthopedist must correct whatever can be corrected in somatic motor functions.

On such foundations and with clear understanding of the nature of the various disorders present, the educators must develop the methodology that will be adaptable individually to the type of dysarthria that is found in a given patient.

How much may be accomplished with such a plan will be decided only by future experience.

BIBLIOGRAPHY

1. Weisenburg: Aphasia, Philadelphia, 1935.
2. Margulis, M. S.: Zur Frage der zerebraben Dysarthrien. *D. Zschr. f. Nvhk.*, 93: 293. 1926.
3. Hoff, H.: Das Stirnhirn und seine Beziehung zu Gang und Sprache. *W. Kl. W.*, 44: 1588. 1931.

VUE SYNTHETIQUE DES PRINCIPALES ACQUISITIONS DE PHYSIOLOGIE ET DE PATHOLOGIE VOCALES DUES A LA LARYNGOSTROBOSCOPIE

Par

RAOUL HUSSON, M. D.

I

INTRODUCTION

La méthode stroboscopique est utilisée pour l'observation de la vibration des cordes vocales pendant la phonation depuis un demi-siècle environ. Son introduction en France ne data cependant que de cinq années, et se rattache essentiellement à l'apparition du Stroborama construit par les Frères Seguin sur les indications du Docteur J. TARNEAUD.

En sus de la connaissance de la quasi-totalité des travaux relatifs à la laryngostroboscopie publiés en langue étrangère, nous nous sommes livrés à des observations systématiques de larynx normaux et malades aux côtés du Docteur J. TARNEAUD depuis 1932, notamment à sa consultation laryngo-phoniatrique de l'hôpital Léopold Bellan. Nous avons également pratiqué des observations laryngostroboscopiques, en collaboration avec le Docteur A. OMBREDANE, Assistant de Psychologie Pathologique à la Sorbonne, à l'Hospice de Bicêtre sur différents sujets atteints de lésions nerveuses diverses: hémiplégiques, pseudo-bulbaires, etc. . . .

En face de l'enthousiasme de certains de ses protagonistes et du dédain systématique de ses détracteurs, il nous a semblé que le bilan objectif et impartial des acquisitions essentielles de physiologie et de pathologie vocales dûes à la laryngostroboscopie pouvait et devait être utilement dressé.

On ne trouvera pas dans les lignes qui suivent un exposé détaillé et complet de toutes les observations et de toutes les déductions que la stroboscopie a permises, relativement à la physio-pathologie de la voix, ce qui nécessiterait un véritable livre qu'il est sans doute encore prématuré d'écrire. Nous nous sommes efforcé uniquement de dégager les grandes idées générales nées de l'observation stroboscopique du larynx en phonation, tant du point de vue des maladies de la voix que de la physiologie normale de la fonction phonatoire.

II

ACQUISITIONS DE PHYSIOLOGIE VOCALE

Première acquisition: Etude détaillée de la vibration des cordes vocales.

Dès l'application de la méthode stroboscopique à l'observation du larynx, on a acquis la preuve expérimentale de la vibration de celles-ci pendant la phonation, fait autrefois nié par un certain nombre d'auteurs tels que SAVART, GUILLEMIN, etc. . . . Les modalités de la vibration des cordes vocales ont pu être étudiées et fixées dans leur détail. Il nous semble inutile de les rappeler ici et nous renverrons pour cela au célèbre travail de MUSEHOLD (1913) ou à l'article écrit par nous et J. TARNEAUD en 1932 sur "La Mécanique des cordes vocales dans la phonation." (Rev. de Laryng. Portmann, N° 8, Sept.-Oct. 1932).

Rappelons seulement que cette vibration s'effectue normalement dans le plan horizontal sans composante verticale appréceable; les cordes vocales ne vibrent donc pas dans le sens du courant d'air, comme des anches vibrantes élastiques. Leur mode vibratoire normal se rapproche plutôt de celui des "anches à bourrelet" imaginées et construites par EWALD dès 1898.

Deuxième acquisition: Réaction des résonateurs sus-laryngés sur la vibration des cordes vocales et conséquences doctrinales.

L'observation stroboscopique du larynx a mis en évidence la réaction exercée pendant la phonation par le résonateur pharyngo-buccal sur la vibration des cordes vocales. Cette réaction a donné lieu à un grand nombre d'observations expérimentales curieuses, telles que le "phénomène paradoxal de TARNEAUD," l'expérience des tubes pré-buccaux de D. WEISS, etc. . . . En outre, nous avons montré que cette réaction donnait la genèse et l'explication des passages et des registres de la voix. (C. R. Acad. Sc. Paris, T. 200, p. 1, 630, 1935).

Des conséquences doctrinales de cette découverte sont considérables. Elle permet de préciser que la cavité pharyngo-buccale n'est pas relancée "in tempo" pendant la phonation, comme l'ont prétendu WILLIS, HERMANN, et d'autres, mais joue le rôle d'un véritable résonateur adjoint au larynx, comme l'avait autrefois affirmé HELMHOLTZ. De plus, ce résonateur forme avec le vibrateur laryngien un *système couplé*, au sens de WEIN et de VOGEL, justiciable des développements théoriques de Lord RAYLEIGH, et dont le degré de couplage dépend du degré de contraction des parois pharyngo-buccales.

Ces conceptions, dûes à divers travaux expérimentaux et à des observations stroboscopiques très fines du comportement des cordes vocales pendant leur vibration, renouvellent entièrement nos idées en maitère de physiologie phonatoire.

Trosième Acquisition: Remise en question de nos idées sur le mécanisme intime de la vibration des cordes vocales.

Les conceptions actuelles du mécanisme intime de la vibration des cordes vocales admettent que celles-ci, en attitude de phonation, sont en état de contraction sous l'action de l'influx nerveux et, en raison de cette contraction, assurent la fermeture de la glotte, en opposant une certaine résistance à la pression sous-glottique. Lorsque cette dernière sous l'action des forces expiratrices, les cordes vocales finissent par être écartées, laissent passer un jet d'air et reviennent en position d'accolement en vertu des forces nées de leur déformation et dûes à l'élasticité propre au tissu musculaire contracté. Il en résulte que leur vibration est un phénomène d'ordre élastique purement périphérique, c'est-à-dire limité à des actions glottiques, l'influx nerveux n'intervenant dans cette action que pour assurer aux cordes vocales un certain degré de contraction en rapport avec la hauteur du son. Mais, lorsque ce degré de contraction est assuré, la vibration se poursuit en vertu d'un automatisme élastique soustrait à l'action de la volonté et entretenu au niveau de la glotte par le courant d'air.

Cette thèse explique parfaitement la vibration des cordes vocales sur le modèle des anches élastiques à bourrelet, dont EWALD et WETHLO ont pu construire de nombreux modèles mécaniques. Cependant, elle est impuissante à rendre compte de très nombreuses particularités de la vibration des cordes vocales que l'examen stroboscopique a révélées. Nous n'en citerons qu'une seule: l'existence de vibrations dites "alternées," signalées pour la première fois par OERTEL, puis de nouveau décrites par D. WEISS.

Dans ce mode vibratoire particulier, la corde droite s'écarte de la ligne glottique médiane alors que la corde gauche s'en rapproche, puis, inversement, la corde gauche s'en écarte lorsque la corde droite s'en rapproche. Autrement dit, les deux cordes vocales vibrent chacune normalement, mais l'une relativement à l'autre avec un décalage de phase égal à une demi-période.

Un tel mode vibratoire ne s'est rencontré jusqu'à présent que chez des sujets atteints de lésions récurrentielles importantes. Cependant, il est totalement inexplicable en admettant l'explication classique de la vibration des cordes vocales par un conflit d'ordre élastique au niveau de la glotte. Dans la thèse classique en effet, les deux cordes vocales doivent s'écarter l'une de l'autre simultanément et se rapprocher l'une de l'autre également dans le même temps. On ne comprend nullement comment une corde vocale pourrait s'écarter de la ligne glottique médiane sous l'influence de la pression sous-glottique, alors que l'autre s'en rapprocherait.

Ainsi donc, l'existence de vibrations alternées des cordes vocales révélées par la stroboscopie est inconciliable avec la thèse classique de la vibration de celles-ci sous l'action de forces élastiques développées au niveau de la glotte.

Par ailleurs, un certain nombre de résultats expérimentaux obtenus sur le chien par BRUIN en Hollande, P. DUMONT en France,

et LINDEMANN en Angleterre, nous ont fait penser que la vibration des cordes vocales était en réalité un phénomène de pure *physiologie nerveuse*, où les forces élastiques développées au niveau de la glotte pouvaient certes intervenir dans l'entretien de la vibration, mais ne jouaient qu'un rôle secondaire, sinon nul, dans la fixation de la hauteur du son.

Des observations stroboscopiques répondant à cette inspiration ont été faites par nous avec le Docteur A. OMBREDANE à l'Hospice de Bicêtre sur une gamme de malades atteints de lésions nerveuses diverses: hémiplégiques, pseudo-bulbaires, etc. . . . Elles nous ont confirmé dans cette voie, et conduit à élaborer une théorie de la vibration des cordes vocales ne faisant intervenir que des données de physiologie nerveuse pour la fixation de la fréquence vibratoire, à laquelle nous avons donné le nom de "théorie neuro-chronaxique de la vibration des cordes vocales." (Comm. Soc. Franç. de Phoniatrie, 21 Octobre 1936.)

Nous ne pouvons nous étendre plus longuement ici sur ce sujet, qui sera développé avec tous les détails désirables par ailleurs. Ajoutons simplement que, dans cette théorie explicative, la vibration des cordes vocales est une activité purement neuro-musculaire de celles-ci, soumise à un contrôle habituel réflexe de niveau bulbaire et, dans le cas d'une fixation volontaire de la hauteur du son, à un contrôle cortical en liaison avec les centres de l'audition.

Quoi qu'il en soit du détail de cette théorie, il n'est pas douteux que l'explication du mécanisme intime de la vibration des cordes vocales ne doit être recherchée sur le plan de la physiologie nerveuse pure, et cette évolution considérable de nos idées sur la question a été sans contredit déclanchée par des apports expérimentaux nouveaux dûs à la laryngostroboscopie.

III

ACQUISITIONS DE PATHOLOGIE VOCALE

Première acquisition: Découverte d'une immense variété de modes vibratoires pathologiques des cordes vocales, traduisant la symptomatologie stroboscopique des parésies laryngées.

Le fait le plus frappant apparaissant lors de l'observation laryngo-stroboscopique de sujets atteints de troubles vocaux divers est l'immense variété des modes vibratoires pathologiques des cordes vocales qui se revèlent.

D'une façon générale, ces comportements pathologiques peuvent recevoir le nom de "troubles de motilité vibratoire" des cordes vocales.

Il est essentiel d'indiquer tout d'abord que ces troubles de motilité sont totalement inobservables au laryngoscope ordinaire, et n'apparaissent qu'à l'observation stroboscopique. Au laryngoscope, les cordes gardent un aspect normal et l'occlusion glottique en phonation paraît également réalisée de façon normale. Cependant,

sous l'éclairage stroboscopique, le comportement des cordes vocales pendant leur vibration paraît troublé selon une grande variété de modalités possibles.

Il ne nous est pas possible de donner ici une description complète de la totalité des modes vibratoires pathologiques qui ont pu être observés et identifiés, ce qui nécessiterait un cadre trop étendu. Nous donnerons seulement une classification qui nous est personnelle de ces modes vibratoires en grandes catégories, répondant d'ailleurs à une hiérarchie dans la gravité de l'atteinte fonctionnelle qui les détermine. Cette classification est la suivante:

a) Troubles de la *phase d'accolement* pendant la vibration.
b) Troubles de *l'amplitude* de la vibration.
c) Troubles de la *fréquence* de la vibration.
d) Troubles du *mode vibratoire*.

Chacune de ces catégories de troubles de motilité vibratoire se subdivise elle-même en un nombre plus ou moins grand de sous-catégories caractérisées par un mode vibratoire pathologique particulier, que nous ne pouvons indiquer ici.

La hiérarchie de ces quatre grandes catégories de troubles est d'ailleurs intéressante à la fois du point de vue pathogénique et du point de vue du pronostic qui peut en être déduit.

Les troubles de la phase d'accolement pendant la vibration se caractérisent par une mauvaise fermeture de la glotte pendant la phase d'accolement des cordes vocales de chaque période vibratoire. En général, le défaut de fermeture siège dans la région aryténoïdienne. Ce trouble traduit un état parétique du système musculaire assurant l'occlusion de la glotte en arriére, notamment de l'interaryténoïdien.

Les troubles de l'amplitude vibratoire traduisent en général un état parétique plus avancé et étendu aux éléments de la musculature glottique intrinsèque, atteignant notamment tout ou partie des fibres musculaires du thyro-aryténoïdien interne. Le trouble peut atteindre la corde gauche uniquement, dont l'amplitude apparaît plus petite qu'à droite, ou la corde droite uniquement, ou les deux cordes vocales à la fois, également ou inégalement; il peut enfin se manifester sur toute la tessiture ou être plus spécialement apparent uniquement en certaines régions de la tessiture, notamment au voisinage des passages de la voix.

Les troubles de la fréquence vibratoire sont constitués par le fait qu'une corde vocale, la droite ou la gauche, vibre à une fréquence plus petite que l'autre. La différence des deux fréquences est en général d'une dizaine de vibrations. Bien entendu, lorsqu'une corde vocale vibre à une fréquence différente de celle de l'autre, elle est vue immobile au stroboscope si le stroboscope est accordé sur la fréquence de l'autre. Dans certains cas, le trouble n'apparaît que sur certains régions de la tessiture. Dans d'autres cas, signalés par GUTZMANN, la corde qui vibre à une fréquence inférieure est tantôt celle de droite, tantôt celle de gauche, suivant la hauteur tonale.

Il est très remarquable et très important de noter que les troubles de l'amplitude et les troubles dela fréquence apparaissent le plus souvent dissociés et indépendants. Dans la majorité des cas, il y a altération de l'amplitude sans altération de la fréquence, ou inversement altération de la fréquence sans aucune modification anormale de l'amplitude. Ceci nous montre que le mécanisme physiologique qui fixe la fréquence de la vibration des cordes vocales est distinct du mécanisme physiologique qui en fixe l'amplitude. Cette constatation est une preuve nouvelle du fait que la fixation de la fréquence vibratoire des cordes vocales pendant la phonation ne répond pas au mécanisme élastique de la thèse classique, car, dans cette dernière thèse, la fréquence et l'amplitude sont toute deux déterminées par les conditions locales de l'équilibre des forces élastiques au niveau de la glotte.

Les troubles de la fréquence vibratoire sont liés à l'intégrité des voies nerveuses qui assurent la transmission des influx moteurs aux éléments musculaires du larynx, et des centres bulbaires et corticaux qui en assurent le contrôle réflexe ou volontaire. A l'appui de cette thèse, l'altération de la fréquence vibratoire d'une corde vocale a été reproduite expérimentalement par Hugo STERN par percussion électrique du récurrent homolatéral.

Les troubles du mode vibratoire constituent le terme ultime de l'altération de la vibration des cordes vocales. Telles sont les vibrations alternées, où les cordes vocales présentent l'une par rapport à l'autre un décalage de phase, et les vibrations où le bord libre des cordes vocales flotte à la façon d'une anche libre. De telles altérations du comportement vibratoire normal des cordes vocales ne s'observent que dans des accidents récurrentiels graves, et sont d'ailleurs accompagnés d'une altération profonde du timbre de la voix.

Pour terminer, insistons encore sur le fait que cette gamme d'atteintes pathologiques des cordes vocales est totalement invisible au laryngoscope ordinaire. Dans tous ces troubles de motilité vibratoire, l'observation laryngoscopique ordinaire ne donne rien, et fait apparaître une image laryngoscopique normale. Seul, l'éclairage stroboscopique révèle le trouble du comportement vibratoire des cordes vocales et permet d'apprécier sa nature et son degré. Il y a donc là une possibilité d'investigation clinique qui échappe totalement à l'examen laryngoscopique ordinaire, et qui permet notamment d'établir le vaste tableau de la *symptomatologie stroboscopique des parésies laryngées* dont nous venons de fixer les grands traits.

Deuxième acquisition: Etablissement de diagnostics précoces dans toutes les maladies de la voix et du larynx.

Comme nous venons de l'indiquer, toute l'énorme classe des troubles de motilité vibratoire des cordes vocales apparaît à la stroboscopie alors que l'observation laryngoscopique ordinaire ne revèle rien. L'observation laryngoscopique ne permet de déceler

une modification du comportement des cordes vocales que lorsque celle-ci est suffisamment intense pour dépasser en grandeur le degré d'imprécision et de "flou" inhérent à l'observation d'un bord libre de corde vocale en vibration.

D'une façon générale, toutes les maladies de la voix et du larynx se traduisent d'abord, pendant une période plus ou moins longue et qui peut atteindre deux ou trois mois, par de simples troubles de motilité vibratoire des cordes vocales, observables seulement à la stroboscopie. Au bout de cette période seulement, le trouble est suffisamment accentué pour être observable au laryngoscope ordinare. L'observation stroboscopique permet donc d'une façon générale le diagnostic précoce de toutes les maladies de la voix et du larynx.

Cette conclusion apparaît déjà dans les travaux de DAHMANN, de Hugo STERN et de Otto HEYMANN. On doit à ce dernier une étude détaillée et systématique d'observations stroboscopiques relatives au diagnostic précoce d'une très grande variété de maladies de la voix et du larynx.

Troisième acquisition: Dépistage précoce des formations nodulaires dans l'évolution des troubles vocaux.

On sait qu'un grand nombre de troubles vocaux évoluent, lorsque certaines conditions dystoniques et dystrophiques sont réalisées au niveau des cordes vocales, vers la formation d'un nodule. Ce nodule siège généralement au tiers antérieur des cordes vocales, et les conditions de sa formation ont fait l'objet d'un travail récent et étendu de J. TARNEAUD.

Résultant de micro-traumatismes des bords libres des cordes vocales pendant leur travail vibratoire, la formation nodulaire débute dans de très nombreux cas par un léger épaississement de la muqueuse du bord libre d'une ou des deux cordes vocales au siège du traumatisme, lequel occasionne tout d'abord une altération extrêmement légère de la rectilinéarité du bord libre de la corde vocale atteinte.

A l'examen laryngoscopique ordinaire, le bord libre des cordes vocales est vu naturellement "flou" puisqu'il est en vibration, et la formation nodulaire n'apparaît que lorsqu'elle est suffisamment développée. A l'examen stroboscopique au contraire, le bord libre de la corde vocale est vu en mouvement ralenti, mais parfaitement net, et toute altération de sa rectiniléarité apparaît dès le début. L'éclairage stroboscopique permet donc le diagnostic précoce des formations nodulaires siégeant sur le bord libre des cordes vocales.

Quatrième acquisition: Mise en évidence de troubles vocaux d'une pathogénie particulière—Troubles réactionnels ou troubles de Tarneaud.

Nous avons indiqué plus haut que nos observations nous ont permis de mettre en évidence, pendant la phonation, une réaction plus ou moins intense des résonateurs sus-laryngés sur la vibration des cordes vocales.

On doit à J. TARNEAUD d'avoir, le premier, montré que cette réaction pouvait, à la longue et dans certaines conditions, déclancher des troubles de motilité vibratoire des cordes vocales. Il a pu montrer en effet que ces troubles apparaissaient en dehors de toute autre cause pathogénique et disparaissaient dès qu'un traitement rééducatif convenable détruisait le désaccord des résonateurs qui occasionnait leur réaction anormale sur la vibration des cordes vocales.

L'étude détaillée de ces troubles réactionnels, faite par J. TARNEAUD, est d'une importance toute particulière en ce qu'elle donne la genèse détaillée des troubles désignés autrefois de façon vague sous le nom de "malmenage vocal."

Les troubles de TARNEAUD, résultant d'un désaccord entre les résonateurs et le larynx, sont à rapproacher des troubles de MOURE, résultant d'une dysharmonie entre le larynx et la soufflerie. Ces deux catégories constituent la classe des troubles dits "fonctionnels" du larynx. On en trouvera une étude détaillée dans le récent ouvrage de J. TARNEAUD, sur "Le Nodule de la corde vocale."

Cinquième acquisition: Dépistage des faux classements vocaux et contrôle des thérapeutiques d'exercice.

Toute mauvaise technique imposée à un organe vocal par un professeur de chant ou de diction se traduit tôt ou tard par l'apparition d'un trouble de motilité vibratoire des cordes vocales.

Dans le cas particulier où une tessiture plus aigue que sa tessiture naturelle est imposée par un professeur à l'organe vocal d'un élève, ce qui est malheureusement courant dans l'état actuel des études de chant, on observe souvent, au bout d'un certain temps, une altération particulière de la vibration des cordes vocales se traduisant par un trouble de la deuxième catégorie ou trouble de l'amplitude de la vibration, découvert initialement et rapporté à sa cause par J. TARNEAUD: les cordes vocales vibrent avec une amplitude réduite et, de plus, leur partie vibrante est telle-même réduite à un certain segment du bord libre des cordes vocales.

Une telle amputation de l'amplitude et de la partie vibrante des cordes vocales a été observée notamment dans le cas de barytons auxquels une tessiture de ténor avait été éducativement imposée. Chez ces sujets, une sorte d'adaptation particulière du larynx s'était produite à la longue, altérant de la façon indiquée le comportement des cordes vocales, mais permettant au sujet de chanter en ténor, bien que naturellement avec une voix de qualité diminuée et une fatigabilité plus grande. Chez deux de ces sujets, une rééducation de la voix dans une tessiture de baryton a pu être poursuivie et elle a en six mois déterminé la récupération progressive des possibilités vocales normales, ainsi que provoqué le retour à la vibration de la totalité de la longueur des cordes vocales avec une amplitude normale.

D'une façon générale, dans le cas de tous les troubles vocaux dits "fonctionnels," qui se traduisent initialement par des dérèglements de motilité vibratoire des cordes vocales observables à l'examen

stroboscopique, le traitement comporte en général une thérapeutique rééducative consistant en exercices vocaux appropriés.

Cette dernière peut aisément et très utilement être contrôlée par l'examen stroboscopique, car elle doit progressivement faire diminuer, puis disparaître, le trouble de motilité vibratoire observé.

Ce contrôle a été préconisé par SERGRE, ainsi que par FROESCHELS et D. WEISS. J. TARNEAUD a de nouveau insisté sur son intérêt et sa nécessité dans son travail sur "Le Nodule de la Corde Vocale."

IV

CONCLUSIONS

Première Conclusion.

Du point de vue de la physiologie normale, il n'est pas niable que l'utilisation de la méthode stroboscopique n'ait permis de faire des découvertes qui, en peu d'années, ont conditionné une réforme complète et profonde de nos connaissances et de nos conceptions relativement à la physiologie de la voix.

Alors que la physiologie phonatoire du larynx se réduisait, dans la théorie ancienne, à un acte d'occlusion de la glotte auquel se superposait un conflit local d'ordre élastique, nous savons maintenant que la voix résulte, au niveau des cordes vocales en attitude d'adduction, d'un acte vibratoire dont la genèse n'est nullement locale. Il existe indubitablement une physiologie spéciale de la vibration des cordes vocales, fait qui fut jusqu'à présent ignoré, et nous savons de plus, grâce à l'analyse stroboscopique, que cet acte vibratoire rentre dans le cadre de la *physiologie nerveuse pure.*

Nous savons en outre que le fonctionnement des résonateurs sus-laryngés n'est pas indifférent à celui du larynx, et que larynx et résonateurs forment un *système couplé,* obéissant à des lois mécanico-acoustiques relevant de la théorie générale des systèmes couplés, et comportant en outre, comme nous l'avons montré, une régulation proprioceptive réflexe.

Ces deux découvertes fondamentales, l'une relative au mécanisme intime de la vibration des cordes vocales, l'autre au fonctionnement d'ensemble de l'organe vocal, conditionnent une révolution totale de nos connaissances en matière de physiologie phonatoire.

Deuxième conclusion.

L'utilisation de l'observation stroboscopique a permis également un enrichissement important de nos connaissances en matière de pathologie vocale.

Dans ce domaine, l'évolution de nos idées est marquée surtout par deux grands faits: en premier lieu, l'existence d'une véritable *pathologie vibratoire* des cordes vocales, qui relève plus de la pathologie nerveuse que de la pathologie musculaire locale; en second lieu la découverte d'une classe importante de troubles vocaux relevant d'une pathogénie nouvelle, d'origine réactionnelle, découverte et analysée par J. TARNEAUD.

Ces faits conduisent à une nouvelle analyse symptomatologique et à une nouvelle synthèse conceptuelle des parésies laryngées d'origine fonctionnelle, et, d'une façon générale, de toutes les atteintes fonctionnelles comprises autrefois sous la dénomination vague de "malmenage vocal."

Troisième conclusion.

Physiologistes et laryngologistes se sont jusqu'à présent partagés en deux catégories relativement à l'opinion qu'ils ont émise sur l'intérêt de la méthode stroboscopique en matière de physiologie et de pathologie phonatoire.

Les uns, à l'image de Hugo STERN, voient dans la laryngo-stroboscopie une méthode de choix présentant une importance de premier ordre et dont la phoniatrie ne saurait plus dorénavant se passer.

Les autres, comprenant bien entendu l'ensemble des laryngologistes n'ayant pas encore utilisé pour des raisons diverses l'exploration stroboscopique du larynx, n'accordent à la laryngo-stroboscopie qu'un intérêt médiocre, sinon même lui dénient toute utilité.

Pour une fois, nous estimons que la vérité n'est pas, à la façon classique, dans un juste milieu arbitraire et trop souvent conventionnel. La fécondité de la laryngo-stroboscopie a largement dépassé les espoirs de ses protagonistes les plus optimistes et les plus enthousiastes. Elle a, an effet, en quelques années, conditionné le renouvellement total de nos connaissances et de nos conceptions en matière de physiologie phonatoire et, relativement aux maladies de la voix, nous montre un domaine encore inexploré riche de ressources symptomatologiques nouvelles et où les conceptions pathogéniques anciennes apparaissent insuffisantes.

Nous devons au surplus nous hâter d'ajouter que les découvertes dûes à la laryngostroboscopie ne sont encore qu'à l'aurore de leur fécondité. La méthode stroboscopique appliquée à l'étude de la physiologie normale et des maladies de la voix réserve, pour de nombreuses années, une riche moisson à tous ceux qu'anime l'amour du travail et de la recherche scientifique.

THE PREPARATORY SET IN STUTTERING

C. Van Riper, Ph.D.
Western State Teachers College, Michigan

A survey of present day methods in the treatment of stuttering finds very few clinicians who content themselves with merely attempting to eradicate the particular case of stuttering which they espouse. Most of us use methods which deal directly with the symptoms of stuttering themselves. Most of us do our best to eliminate and keep out the stutterer's fear of words, and we put a premium on techniques which enable him to keep out and avoid the actual spasms. We build up his confidence, and we measure the success of our treatment in terms of the decreased number of spasms in that stutterer's speech. The permanent results of such therapy are discouraging, and I believe that in the near future we will base our treatment on a totally different set of principles.

One of these principles will demand that therapy be devoted not to the immediate elimination of stuttering symptoms and fears, but to their modification, and to their use. We should seek to change the form of the spasm or the spasm pattern. Gradual change, rather than immediate elimination, should be our aim. If we can change the form of the spasm so that little interruption or abnormality occurs, we have done much. We must teach the stutterer to use his fears and his blocks to rid himself of his handicap.

How can this be done? The answer is that we must use the stutterer's preparatory set as the tool for changing the spasm pattern. Before the nature of these preparatory sets is discussed, it would seem wise to describe what is meant by a spasm pattern. Simply stated, a spasm pattern is a stutterer's characteristic sequence of communicative abnormality. It is the time pattern of what he does when he stutters. Most adult stutterers have at least one spasm pattern; some of them have several. The outstanding characteristics of these patterns are their stereotypy and organization.

We know several things about these spasm patterns. We know that at onset of stuttering the child's symptoms consist of rapid effortless repetition and slight, unforced prolongations, or, as Bleumel has termed them, the primary symptoms of stuttering. We know that as these symptoms become unpleasant to the child he develops awareness and expectancy reactions. He begins to use releasing devices. He begins to react to expectancy of stuttering by employing starters, devices of avoidance, or postponement, or anti-expectancy.

These devices, though at first deliberate and voluntary, soon become habitual and automatic, and are incorporated into the spasm pattern. They give to the stutterer's speech a characteristicness which distinguishes his stutter from another's. Thus he comes to possess in his speech not only the true symptoms of stuttering, but the bad reaction habits of the secondary symptoms.

Few of us seem to recognize the truth that keeps hanging us over our theoretical heads. All that the stutterer does in the so-called spasm cannot be called stuttering. If this man's facial tic, and that man's swallow, and this girl's use of ta-ta-ta, and that girl's tongue protrusion are all stuttering, then stuttering is nothing but a wastebasket term. But it is no wastebasket term! It is a unitary disorder. The primary symptoms, the so-called tonic and clonic blocks, are common to all stutterers. On the other hand, the secondary symptoms, the habitual starters, the decayed devices of avoidance, postponement, and release, are what account for all the hodge-podge and variety of symptoms.

Thus, the typical spasm pattern has the following sequence: (1) The occurrence of expectancy of stuttering unpleasantness on an approaching word or sound. (2) The use of starters or habitual devices of avoidance, postponement, anti-expectancy. (3) The speech attempt. (4) The occurrence of primary symptoms. (5) The use of release devices. Our job is, therefore, to use (1) the expectancy of stuttering and (2) the primary symptoms, in such a way as to change the form of the spasm to one which will have less abnormality and interruption. This will itself decrease the fears and unpleasantness and reduce the frequency of spasms. But how can this be done?

It is this writer's contention that in stuttering the preparatory sets determine the form of the stuttering block or spasm and are the cause of his feeling of compulsion. It is odd that one of the most interesting and promising phases of the problem of stuttering, the part played by preparatory sets, has received so little attention from speech pathologists. The phenomena and functioning of preparatory sets are to be found in all forms of voluntary behavior. The runner, crouched in preparation for the pistol shot, the car driver awaiting the green "go" signal, the typist and the pianist in activity illustrate their importance.

Before citing evidence in support of this thesis, it would seem wise to define and describe the preparatory set. In psychology many different terms have been employed to name it. It has been called the aufgabe, the einstellung, the determining tendency, the attitudinal set, and all of these may be included in the description of a preparatory set *as a pre-stimulus neuromuscular adjustment which selects, determines and controls the response to the expected stimulus.* As in foot-racing or reaction time experiments, there is usually (1) a warning signal which gives rise to (2) a neuromuscular adjustment consisting of tiny rehearsal movements or patterns of appropriate muscle tensions (tonus) which, when (3) the expected signal occurs,

result immediately and involuntarily in (4) the pre-determined response.

In any stuttering block all the above conditions are usually present. A certain cue, (1) the perception of the word, or one of its sounds as being difficult, acts as a warning signal to arouse the specific expectancy of stuttering on that word. (2) This specific expectancy is no vague abstraction but consists mainly of tiny rehearsal movements and increases in tonus of the musculatures to be used in the stutterer's characteristic way of stuttering or spasm pattern. (3) Always there is the expected signal, the feeling of block or "getting stuck" or of finding himself repeating or prolonging a vocal or articulatory posture. When this occurs the preparatory set is released and the overt performance follows in its characteristic fashion.

The stutterer feels that he has no control over this stuttering performance, that he is seized by a compulsion to react in this abnormal way. The reason for this commonest of all stuttering experiences is that the stutterer does indeed have a compulsion to react so peculiarly, and that he can neither control not help himself at the moment. But the reason for his inability and compulsion lies in the power of the preparatory set to determine and control the response to a signal. The runner with a preparatory set to run at the command will run even though the starter cries "jump."

In previous publications we have shown that the form of the stuttering spasm is largely the results of devices the stutterer has used to avoid, minimize and release his primary symptoms, and that when these devices become habitual they form rather stereotyped spasm-patterns. Many other writers have pointed out that most stutterers have distinct and characteristic spasm-patterns. It seems to the author that the preparatory set is the mechanism which permits us to distinguish one stutterer from another, by his mannerisms of stuttering. All stereotyped reaction patterns acquire much of their consistency by remodeling their preliminary motor adjustments, and it is not too much to expect that similar restructuring of the preparatory set occurs in stuttering. The form of the overt stuttering is laid down and determined before the speech attempt is made.

Some of the objective evidence for the existence of these preparatory sets come from a study of the breathing abnormalities of stutterers when speaking insolated words. Often, prior to the signal to speak and before an overt attempt was made, the thoracic breathing of stutterers showed the same pattern of breathing abnormality which occurred when the speech attempt was actually made.

In an experiment, the report of which has been published in the September, 1936, journal of this society, single words were exposed and the stutterer pressed a polygraph key to signal expectancy or non-expectancy of stuttering. A certain interval, from 2–10 seconds, elapsed before signal to say the word was given. Breathing records

showed that often the stutterer's breathing showed a pattern of abnormality very similar to that which actually occurred in the breathing when the word was spoken. (Slides will demonstrate these rehearsal phenomena.) The stutterer, it seems, plans and determines the duration and form of his spasm before he ever attempts the feared word. This pre-determination of an action is what we call the preparatory set in stuttering.

Other small rehearsing movements were frequently noted in the protocols as occurring in other speech musculatures.

However, as has been suggested, the preparatory set need not include actual outward rehearsal but may be carried by patterns of posture or muscle tonus. Very little research has been done in psychology to measure these tonus patterns. Freeman has shown that during expectancy of shock the muscle tension was greatest in the part in which the stimulation was expected; and that anticipatory tension patterns for motor reactions remained the same upon subsequent repetitions. Strother has shown increases in muscle tones (through action current recordings of the response of the masseter muscles) to occur prior to speech attempt. Fletcher and Robbins have shown marked changes in plethysmocraphic recordings during the expectancy period.

Beside the objective testimony above cited, the following subjective reports are of interest. Twenty out of twenty-five stutterers reported rehearsal movements during the period between word exposure and speech attempt. All 25 subjects reported increased tension in various speech musculatures prior to speech attempt on feared words but not on non-feared words.

In an intensive introspection experiment using Ach's method of fractionation, as yet unpublished, and a multiple choice questionnaire, reports of rehearsal movements prior to speech attempt were obtained from all subjects.

Two other completed experiments which have been accepted for publication throw some light on these preparatory sets. In the first, the stutterer was required to read aloud, stopping instantly and protruding his abdomen at the sound of a buzzer. A pneumograph operated onto a polygraph, and provided objective measures of reaction time and breathing. The buzzer was rung during stuttering block. It was found that the stutterer could stop and protrude his abdomen as quickly during spasm as during free speech, although silence provided quicker response. In this instance, at least, the stutterer's compulsion to stutter could hardly be due to a neurological block that lasted for the duration of the overt spasm. With a preparatory set to stop at a certain signal, the stutterer was perfectly able to do so.

In another experiment, using approximately the same apparatus, single words were exposed to the stutterer and he was required to predict the duration of the expected spasm. The signal to speak was then given and the duration of the spasm was measured. It was

found that most of the stutterers were able to predict very accurately whether their spasms were to be of long, average, or short duration. One legitimate explanation of this result is that a preparatory set or plan existed prior to speech attempt, and that this preparatory set determined the duration of the spasm.

Finally, in the course of experiments in stuttering therapy at the University of Iowa and Western State Teachers College, Michigan, it was found possible to produce various types of stuttering reactions by teaching different preparatory sets. In 10 out of the 12 stutterers with whom the procedure was attempted the experimenter was able to control the reaction by controlling the conscious preparatory set. Moreover, it was found that as these preparatory sets were assumed time after time upon the appearance of a feared word, they tended to become habitual and less conscious until at length the new reaction occurred often without any voluntary assumption of the new preparatory set. The significance of these findings for stuttering therapy is obvious. Instead of seeking immediately to eliminate the stuttering spasm, let us rather modify it. Let us change it to a form which carries little social penalty so that the individual can stutter and yet not suffer. Since fear of stuttering seems to beget and precipitate a great many of the blocks, and since fear is largely the expectation of the unpleasant, let us change the form of stuttering to a relatively pleasant performance. This would seem to require a spasm-pattern which has little vocal abnormality and interruption to communication. Thus, expectation would be followed by a successful and relatively pleasant reaction and thus fear—the expectation of the unpleasant—tends to disappear, and the number of blocks decreases.

I believe that in the future our therapy will be devoted not to the elimination of stuttering symptoms, but to their modification and change. Much of the handicap is due to the contortions and vocal abnormalities of the secondary symptoms. Let us break up these old reaction habits by breaking up their preparatory sets. Let us say to the stutterer: Stutter all you wish to—but stutter in this way—and then let us give him a way in which there is little abnormality and little interruption. Let us teach him to get set to stutter in this way. Then, if we make the primary symptoms of stuttering the signals which set off this preparatory set, the reaction will occur automatically.

No doubt there is much tendency to wonder whether such a therapy is possible. For several years the speech clinics of the University of Iowa and Western State Teachers College have been experimenting with various therapies devoted to the symptoms of stuttering as well as to the causes. The one upon which we have been working is that which has named itself the Stop-go. It would take too long to describe it in detail, but essentially it consists in the building up of a preparatory set to attempt to say the feared word as a whole, but to get in readiness to react to a felt block by stopping immediately and then finishing the rest of the word. You notice

that the primary symptoms are used as signals. They are the pistol shot that sets off the runner's lunge. Thus we use stuttering to get rid of stuttering. We teach stutterers to hunt for expectancy, for feared words or sounds; and to prepare for that quick stop-and-go-reaction. We find that these sets become habitual associates of feared words, even as did the old ones. We find that the reaction occurs automatically, and that instead of a stutterer having a spasm pattern like "a-a-a, thhth-this," he has one like "th-is." The latter is relatively so pleasant that fear goes out the window, for fear is the expectation of the unpleasant. The number of blocks, as several recent experiments have shown, is proportional to the expected penalty. Since little penalty attaches itself to so short and so unnoticeable a block, the number of blocks decreases.

A word of warning is vitally necessary. The stutterer must be taught not merely to split the word whenever fear comes. He must plan always to say the word as a whole, and to get set to react with the stop-go only when an actual block is felt. The block, not the fear, must be the signal which sets off the preparatory set.

We have worked out a great many devices and techniques for tearing down the old preparatory sets and building up the new stop-go set, but time does not permit their inclusion in this paper. Suffice it to say that these are the principles we follow: (1) Get rid of the fundamental cause of the primary symptoms, whether it be shift of handedness or something else, (2) Change the form of the spasm to one which is neither peculiar nor of long duration, (3) Tear down the old secondary symptoms of avoidance, postponement, starting and release, (4) Use the primary symptoms to set off the stop-go preparatory set, (5) Seek out feared words so as to associate with them these new preparatory sets.

May we again stress the fact that this symptomatic therapy, so briefly and perhaps prematurely sketched above, is designed to serve only as a supplementary aid to treatment based on etiology. As such, it has proved its value in actual clinical practice.

SPEECH TRAINING IN THE CHILD CRIPPLED BY SPASTIC PARALYSIS

ROBERT L. CARROLL, M. D.,
Orthopaedic Hospital, Los Angeles, California

What is the relation of the muscles of speech to muscles of the extremities? Will speech training alone accomplish the best results in speech improvement? With whom should the speech trainer co-operate?

First, let us consider the pathology of the child with spastic paralysis. This is usually believed to be the result of birth injury, a hemorrhage in the brain, and though this may frequently be the cause, it is not always necessarily so. If it is a hemorrhage the pressure on the adjacent nerve cells destroy them. There may be a rupture of a rather large blood vessel and a blood clot formed, but this is rather rare, and usually it is supposed to be many small—even pin-point hemorrhages throughout the affected part of the brain. When these nerve cells are destroyed, their control over the lower motor neuron in the spinal cord is gone and the extent of the injury determines the extent of the paralysis which may be in one or more or even in all extremities. However, trauma is not always the cause, for this condition has been noted in babies delivered by Caesarian section, before there was any pressure on the head. And the cause of the trouble has been found to be failure of proper development.

The sensory or afferent stimuli pour in from all over the body through stimulation of the skin, joints, muscles, tendons or through eyes and ears. If these stimuli cannot be controlled, they stimulate an efferent overflow through the motor area producing what may be termed an "explosion" of movements. This may be likened to an irrigation ditch. If the force of the water is controlled, and only a normal amount is allowed to flow through the ditch into the smaller irrigation furrows, the water flows smoothly under direct control and goes where it is supposed to go, but if the force is not properly controlled and a flood gate is opened whereby the irrigation ditch is allowed to overflow, then the water will rush madly over the whole field instead of through the proper channels; so by cutting down the amount of water allowed in the ditch, better control is

[1]Read before the Western Association of Teachers of Speech, November, 1936, at Pasadena, Calif.

gained. By cutting down the sensory stimuli coming into the central nervous system, better control is gained. This, of course, is gained by keeping away loud noises which are very stimulating, and keeping the patient from becoming excited through what he may see as well as hear; and stabilizing the joints with braces will cut down the sensory stimuli that are sent into the central nervous system by reducing joint motion; hence the indication for braces and casts for physiological reasons. It was noticed years ago at the Orthopaedic Hospital, by Dr. Lowman together with the late Dr. Stivers, both of whom were greatly interested in the spastic child, that when a child with spastic quadraplegia was in a cast to hold the lower extremities still, movements of the upper extremities were less and were better controlled.

It has been aptly said "there is no motion without emotion." Hence the necessity, especially in the spastic child, to control the motions so as to better control emotion. Speech is only one phase of expression, and it is natural, in some people particularly, for gesture to precede speech. The motor area for the upper extremities and the speech center are closely associated in the brain. Hence, the association of gesture with speech, and Dr. Stivers found that speech training was easier when the patient would use his arms or fingers to make the same letter at the same time he tried to pronounce it, such as making the letters "o" or "a," and that certain arm exercises are very beneficial when used with speech exercises. Others have recently found in teaching non-readers to read, that they could remember the words when they traced them with the hand, thereby perceiving through the sensory kinesthetic route.

In speech training, of course, it has long been noticed that the response of the upper extremities is much greater than that of the lower extremities, yet there has been noticed definite improvement in speech with improvement in walking. The voluntary muscles of speech may be involved in the same way as the voluntary muscles of locomotion or upper extremities, hence anything that helps to control one set of voluntary muscles will also help to control another set of the same kind of muscles.

Inco-ordination is the failure of Sherrington's law, the double innervation of muscles; that is, when there is a message sent to one group of muscles to flex a joint, another message normally should be sent to the opposing group to relax, but in the spastic child, we all know how quickly they contract both sets of muscles instead of relaxing one set, and inco-ordination in speech occurs in the same manner as inco-ordination in walking. Anything that helps the patient to relax also helps him to get better control of his voluntary muscles, and it has been noticed that a patient has much better control, in his speech training, after treatment in the warm pool where the patient relaxes and gets better control over the muscles of his extremities through the sedation of heat rather than stimulation, which would be caused by cold water. The more the stimulation the less the co-ordination.

In the gymnasium the physical education teacher should seek sensory control first and range of motion should be a secondary consideration. The physical education teacher, the speech teacher and the doctor should all work together along the same lines to help the patient get better control, and each will help the other. It may be necessary for the doctor to help get sedation through medicine, without which the patient would not progress as rapidly. An orthodontist might also be consulted to be sure that the patient is not making too many movements that would tend to compress the jaws and make them narrow and possibly cause malocclusion. If the pull of a little rubber band can change the position of a tooth over a period of time, certainly the force of the combined muscles about the lips and cheeks, if used to excess, can exert an unwanted pressure which may cause some narrowing of the jaws as mentioned.

SUMMARY

The voluntary muscles of speech are of the same nature as the voluntary muscles of locomotion, and are subject to the same relaxation and control influences. Speech training alone will not improve the speech as well or as rapidly as speech training in conjunction with other measures to improve other muscular action and co-ordination of the patient as a whole. Hence the necessity is shown for co-operation between the speech teacher, the physical education teacher, the doctor and the orthodontist. These unfortunate patients are entitled to every advantage that scientific training can give through co-operation with others working in this field.

BOOK REVIEWS

By CHARLES H. VOELKER
Director, Dartmouth College Speech Clinic

PHONETICS FROM THE POINT OF VIEW OF ENGLISH SPEECH. By C. F. KANTNER (Louisiana State University) and ROBERT WEST (University of Wisconsin). Madison: College Typing Company, pp. 212, 1936. $3.50.

West and Kantner first published *Kinesiologic Phonetics* in 1933. This reprint, *Phonetics*, is an enlargement of that book. In the main the material of the earlier book is given in *Chapter IV* of the new edition. In addition there are four new chapters, *The Symbolization of Speech Sounds, An Introduction to the Study of Phonetics, A Brief Review of the Speech Mechanism*, and *Phonetic Metamorphology*. This last chapter, dealing with material in the field of the linguist, is divided into *Acoustic Considerations* and *Physiologic Considerations*. It contains considerable material on comparative phonetics, which is essential to the therapy of barbaralalia.

PROBLEM CHILDREN. By J. E. BENTLEY (American University). New York: W. W. Norton & Co., pp. xxiii and 437, 1936. $3.50.

The author has compiled a great deal of practical information and has presented it in a very readable style. *Part One* concerns *The Physical Disabilities of Problem Children; Part Two, The Psychological Approach to the Study of Problem Children; Part Three, The Social Disabilities of Problem Children; Part Four, The Educational Disabilities of Problem Children.* The book gives a very rounded picture of special education. *Chapter IV* on *Speech and Its Disabilities* is written in excellent style.

THE LIFE OF A LARYNGECTOMEE. By N. EHRLICH (Dir. and Lecturer, Lost Speech Alliance, Vanderbilt Clinic, N. Y. C.). New York: Froben Press, pp. 33, 1937. $1.00.

The Lost Speech Alliance is the organization of the Presbyterian Hospital Speech Clinic, New York City, for postlaryngectomy patients who wish to learn to speak with an artificial larynx. Admission to the clinic is as follows: "The introduction of an attending physician about the throat condition is required for admission. There is a registration fee for new members, for this four months' course, to cover the cost of postage and printing. Those who are unable to pay may arrange admittance through the Social Service Department." Ehrlich, who must use an artificial larynx himself, says: "Attention to the surgeons (Laryngectomers) and their patients (Laryngectomees). We advisedly coin these names." The brochure gives interesting information on personal details in the handling of these patients.

PROGRAM

The American Speech Correction Association

CHICAGO, ILLINOIS
October 11, 12, 13, 1937

MONDAY, OCTOBER 11th

MORNING

8:00. Registration begins.

9:00. THE PRESIDENT'S ADDRESS
MAX A. GOLDSTEIN, M. D., Central Institute for the Deaf.

STUDIES IN STUTTERING

9:30. *"The Role of Evaluation in Stuttering Behavior."* (30 min.),
WENDELL JOHNSON, Ph. D., University of Iowa

10:15. *"Overt Stuttering in Relation to Expectation of Stuttering."* (30 min.),
ROBERT MILISEN, Ph. D., Indiana University

11:00. *"Stuttering with Relation to Word Accent and Word Position,"*
SPENCER F. BROWN, Ph.D., Ohio State University

(Time for discussion)

AFTERNOON

2:00. *"Brain Potential Studies of Stuttering."* (30 min.),
LEE EDWARD TRAVIS, Ph.D., and JOHN R. KNOTT, M. A.,
University of Iowa

2:45. *"The Role of Visual Cues in Stuttering."* (30 min.),
JAMES MADDOX, M. A., Purdue University

3:30. *"The Nature and Effect of the Stutterer's Avoidance Reactions."* (30 min.),
MELBA KIMMELL, M. A., University of Iowa

(Time for discussion)

EVENING

7:00. Annual Dinner, Club Lounge.

TUESDAY, OCTOBER 12th

MORNING

9:00. *"Tension and Facial Appearance in Stuttering."* (30 min.),
JOHN SNIDECOR, M. A., University of Idaho

9:45. *"What is the Common Denominator in Twinning and Stuttering?"*
(30 min.)....MILDRED F. BERRY, Rockford College, Rockford, Illinois

10:30. *"Methods of Tearing Down the Adult Stutterer's Spasm Patterns."*
(30 min.)..C. VAN RIPER, Ph.D., Western State Teachers College, Mich.

11:15. *"Hearing Deficiencies in Relation to Defective Speech."*
HORACE, NEWHART, M. D., Minneapolis, Minnesota

AFTERNOON

2:00. *"The Incidence of Utterances Concerning Sex Organs and Sex Functions in a Two-Year Speech Record of Two Pre-School Children."* (30 min.),
A. A. LOW, M. D., University of Illinois,
Dept. Psychiatry, College of Medicine

2:45. *"A Co-operative Program for Attempting to Improve the Abnormal Voice Arising from Inadequate Palate."* (30 min.),
ELMER L. KENYON, M. D., Chicago, Illinois

3:45. *" 'Two Room Technique' for the Treatment of Stuttering."* (30 min.),
RAYMOND CARHART, M. D., Northwestern University,
Department Speech Re-Education

4:30. *"The Effect of Cleft Palate Operations on Speech and Voice,"*
G. M. DORRANCE, M. D., Philadelphia, Pennsylvania

(Time for discussion)

EVENING—8 o'clock

Joint Session with the American Academy of Ophthalmology and Otolaryngology

This very important group will meet at the same time as the Association.

Lee W. Dean, M.D., president of the Academy, and Max A. Goldstein, M.D., president of the Speech Correction Association, have arranged a joint session of the two Societies for Tuesday evening, at eight o'clock, in the Grand Ball Room.

All our members are most cordially invited to attend, as an interesting program has been arranged.

Presiding:

LEE WALLACE DEAN, M.D.,
President, American Academy of Ophthalmology and Otolaryngology

MAX A. GOLDSTEIN, M.D.,
President, American Speech Correction Association

PROGRAM

STAMMERING:

Its Psychology.....MEYER SOLOMON, M.D., Chicago, Illinois
Its Treatment.....ELMER L. KENYON, M.D., Chicago, Illinois
Its Prognosis.....BRYNG BRYNGELSON, Ph.D., University of Minnesota

CLEFT PALATE:

Its Surgery.....VILRAY P. BLAIR, M.D., St. Louis, Missouri

Its Pedagogy.....HERBERT KOEPP-BAKER, Pennsylvania State College

APHONIA:

.....ROBERT WEST, Ph.D., University of Wisconsin

APHASIA:

General Consideration.....G. OSCAR RUSSELL, Ph.D., Ohio State University, Columbus

Congenital Aphasia.....MISS MILDRED MCGINNIS' Central Institute for the Deaf, St. Louis, Missouri

COGNATE SPEECH DEFECTS:

.....CHARLES S. BLUEMEL, M.D., Denver, Colorado

LABORATORY RESEARCH OF THE SPEECH PATHOLOGIST:

.....LEE EDWARD TRAVIS, Ph. D., Iowa State University, Ames, Ia.

THE OTOLOGIST AND THE SPEECH PATHOLOGIST:

MAX A. GOLDSTEIN, M.D., Central Institute of the Deaf, St. Louis, Missouri

GENERAL DISCUSSION

To be opened by:

SMILEY BLANTON, M. D., New York, N. Y.

SAMUEL D. ROBBINS, A.M., Boston, Mass.

(All papers in the above Symposium will be limited to 10 minutes each)

WEDNESDAY, OCTOBER 13th

MORNING

9:00. *"The Relationship Between Psychological and Physical Measures of Masculinity."* (30 min.).....HOWARD GILKINSON, Ph.D., University of Minnesota

9:45. Round Table Demonstration and Discussion: *"Childhood and Pre-School Speech Problems."* (30 min.),
SARA S. HAWK, Ph.D., HAZEL DUCLES MUELLER, M.S., CLARA B. STODDARD, B.S., AND LETITIA B. RAUBICHECK, Ph.D.

10:15. *"Motion Picture Studies of the Vocal Bands and Vocal Attack."*
PAUL MOORE, Ph.D., Northwestern University

11:00. *"The Mechanism of the Action of the Larynx as Shown by Glottal Lip Motion Pictures,"*
JOEL J. PRESSMAN, M.D., Cedars of Lebanon Hospital, Los Angeles, California

---

AFTERNOON

2:00. *"Reading and Speech."* (30 min.),
HELEN GIBBONS, University of Minnesota

2:45. Business Meeting.

SHIFTED PROGRAMS

It will be noted that over and above the thirty minutes granted each paper, 15 minutes is allowed for discussion and an additional half hour at the close of each session for continuance thereof. If the chairman so desires, he may call for a vote of those present at the morning sessions of Monday and Wednesday as to whether they desire to condense these discussion periods and thus move forward the afternoon papers to free those sessions so all may attend the Motion Picture Sections of the Academy, which are as follows:

OTOLARYNGOLOGY

MONDAY, OCTOBER 11—2:00 P. M.

(Red Lacquer Room)

Presiding: WILSON O. JOHNSTON, M.D., Second Vice-President

MOTION PICTURE PROGRAM

1. *Bronchoscopy and Œsophagoscopy on the Cadaver and the Living,*
   SAMUEL IGLAUER, M.D., Cincinnati, Ohio
2. *The Larynx—Demonstrating Position of Cords in Sounding Low and High Tones*.....JOEL J. PRESSMAN, M.D., Los Angeles, California
3. *Colored Photography of the Larynx Under Local Anaesthetic, Lynch's Method*.....DEAN M. LIERLE, M.D., Iowa City, Iowa
4. *Laryngectomy—One Stage* (colored), ten minutes.
   LOUIS H. CLERF, M.D., Philadelphia, Pennsylvania
5. *Movietone of Laryngectomized Patient.*
   J. C. BECK, M.D., AND M. REESE GUTTMAN, M.D., Chicago, Illinois
6. *Pansinusectomy*.....W. A. WAGNER, M.D., New Orleans, Louisiana
7. *Two Plastic Operations:*
   A. *Congenital Nasal Deformity—Hinged Bone Graft.*
   B. *Ankylosis of the Lower Jaw—Anthroplasty.*
   (15 minutes)
   GORDON B. NEW, M.D., Rochester, Minnesota

WEDNESDAY, OCTOBER 13—2:00 P. M.

(Red Lacquer Room)

New Instruments—Between Reels

Presiding: WILSON O. JOHNSTON, M.D., Second Vice-President

MOTION PICTURE PROGRAM

8. *The Management of Hypertrophied Nose,*
   C. R. STRAATSMA, M.D., New York, N. Y.
9. *Rhinoplasty*.....C. L. STRAITH, M.D., Detroit, Michigan
   (By invitation)
10. *Rhinoscleroma by Dr. Giffen, American Mission Hospital, Assuit, Egypt.*
    Presented through the Otolaryngic Registry, sponsored by the O. O. R. L. Academy at the Army Medical Museum, Washington, D. C.
    J. E. ASH, M.D., Washington, Lt. Col. Medical Corps, U. S. A., Curator
    (By invitation)
11. *Treatment of Cicatricial Stenosis of Œsophagus,*
    J. A. FLYNN, M.D., Washington, D. C.
12. *a. Operated Pigeon, Cat and Robin Labyrinths.*
    b. Japaneze Waltzing Mice.
    c. Pathology of Temporal Bone (colored).
    DOROTHY WOLFF
    (By invitation)

ON THE CORRECTION OF FOREIGN BROGUE

Breath Stream Control During Transitions in French[1]

JAMES L. BARKER,
University of Utah, Salt Lake City, Utah

Most phoneticians have set for themselves the task of discovering facts about the pronunciation of a single language, usually their mother tongue, without any reference to other languages. It is true, they frequently make comparisons from language to language, but in so doing they utilize their own discoveries for the mother tongue and the published discoveries of others for other languages. None of the better known phoneticians has ever published conclusions based on a comparative experimental study of two or more languages. Yet that is precisely the type of study on which the correction of foreign accent must be based.

The investigations of most, if not all phoneticians, have been predicated on the basic theory that slight differences in position are responsible for the differences in pronunciation, and consequent foreign brogue. The earlier phoneticians determined these positions by direct observation and rough measurement. By means of the artificial palate the Abbé Rousselot determined the positions with great accuracy, and he thought that in the artificial palate he had found a reliable means of investigation and at the same time an unfailing means for the correction of faulty pronunciation.

The phoneticians seem to ask: What else can determine the difference in sound (pronunciation) from language to language? Is not the effort to produce sound and to expel the breath continuous within the breath group?[2] Are not syllables unreal and without objective existence in normal continuous speech?[3] Is not the

---

[1]This article is by way of reply to Professor C. E. Parmenter and Dr. S. N. Treviño who begin an article in the June number, 1936, of the PMLA thus:

"It has been stated repeatedly by Professor James L. Barker of the University of Utah that in French the breath stream is interrupted in certain cases after the production of a sound and that the position for the following sound is taken in silence during the interruption. The cases mentioned specifically in which such interruptions occur are the transitions from consonant to vowel, from vowel to consonant, and from consonant to consonant."

And they conclude the article as follows:

"The evidence presented in this paper leads to the conclusion that there is no interruption of the breath stream in passing from consonant to vowel, from vowel to consonant and from consonant to consonant within the phonic group in French."

[2]Sweet, Panconcelli-Calzia, Passy, Grammont.

[3]Panconcelli-Calzia.

transition from one sound to another determined by the law of least effort and the shortest path? And are not transitions made in the same manner in all languages, and of negligible importance?[4] What then deserves attention? Evidently the positions required for the various vowels and consonants as such without regard to the manner in which these positions are taken and left and without regard to the control of the breath in relation to the taking and leaving of the positions. In any case the control of the breath, since the effort to expel it is continuous, would be the same in all languages, and since this is true, it is not a variable factor from language to language, and there is no practical utility in a comparative study of it.

In their denial of the Barker theories Professor Parmenter and Dr. Treviño are led of necessity to the acceptance of the above theories.

In reaching their conclusions they made use of the kymograph and the oscillograph. In reaching my conclusions I made use of the artificial palate and the kymograph, and in this article I am publishing for the first time experiments with the oscillograph.

Though Professor Parmenter and Dr. Treviño do not offer any experiments with the artificial palate, they must place their chief reliance on it for the determination of differences in position. In common with other phoneticians adhering to the theory of no breaks in the breath stream within the breath group, they have advanced no other explanation of the difference in acoustic effect in different languages, as in the case of **l** in French and **l** in English, except the difference in position revealed by the use of the artificial palate. Though it is true that differences in position do exist, in 1912 I demonstrated repeatedly to the Abbé Rousselot and to his laboratory assistant, Dr. Chlumsky, that both French and English **l** can be easily produced with the tip of the tongue in any forward position. In fact, I repeatedly pronounced French **l** in the so-called English position and English **l** in the so-called French position and thus demonstrated that the difference in position is not the cause of the characteristic acoustic difference.[5] In *Mechanism* and *Position*[6]

---

[4]Paul Passy.

[5]In the *Revue de Phonétique*, dated 1914, but appearing after the War, Dr. Joseph Chlumsky published an article on "Les Consonnes Anglaises Comparées aux Consonnes Françaises à l'Aide du Palais Artificiel et de l'Observation Directe," in which he concludes: "Ce n'est donc pas le point d'articulation qui distingue ce **t** du **t** français, ni n'en explique le caractère acoustique," but that "dans l'articulation des **t** anglais et français, la pointe de la langue fonctionne de façon différente, verticalement en anglais, horizontalement en français. C'est là ce qui produit la divergence acoustique si nettement marquée," and he draws the same conclusion concerning other consonants, for example, **l**.

The difference in sound is not caused by the slight difference in tongue position. With the use of the artificial palate, Dr. Chlumsky saw me demonstrate to the Abbé Rousselot in 1912 and 1913 that French **l** could be pronounced in the "English position" and English **l** in the "French position." Hence the difference is sound could not be due to a slight difference in position. (See *End-Consonants and Breath-Control in French and English*, Modern Philology, 1916.)

What is original in Dr. Chlumsky's article is the theory that the difference in sound is due to the articulation of the front consonants with the tongue in a

I pointed out that all minor differences in position are not the causes of characteristic acoustic differences but are themselves the results of differences in mechanism.

If either the theory of continuous expulsion of breath and production of sound, or of breaks or interruptions in the effort to expel the breath is to be accepted, it should be capable of experimental demonstration. Such demonstration need not be direct; it may be indirect. The demonstration to be complete should show that the theory is not in conflict with known phenomena and that it may be used to solve the problem of the correction of faulty accent.

In respect to its application to the correction of faulty accent, the theory of the continuous expulsion of the breath has signally failed and has even brought phonetics into disrepute. One of the greatest phoneticians of all time, the Abbé Rousselot, during a period of five years, did not succeed in a single case in eliminating English accent in the pronunciation of French.[7] He made extensive use of the artificial palate in the determination and correction of differences in position, but the theory that the expulsion of breath is continuous within the breath group and that accent may be corrected by correcting differences in position failed to stand the practical test. Others have had no better success in the application of this theory to the correction of accent.

How can the theory stand the practical test? Read the texts based on this theory—Rousselot, Grammont, Navarro-Tomás, etc.—and examine carefully their descriptions of **l, r,** etc., and in nearly all cases, what is said of French **r** may be said of the corresponding dialectual variety of English **r,** and except for the difference of position what is said of French **l** may be said of English **l,** and yet this one difference of position is inconsequential, since either French **l** or English **l** is easily pronounced throughout the whole range of positions in which any **l** is pronounced.[8]

Professor Parmenter will probably admit that the Barker theories do meet the practical test successfully. He has seen drills based on the Barker theories used in the teaching of pronunciation at the University of Chicago and perhaps would not dispute the fact that such drills are very effective in the teaching of French pronunciation

---

vertical position in English and a horizontal position in French. Direct observation should have shown that this theory is not correct. If one will observe the tongue in the pronunciation of the word *milk*, the manner of taking the position for **l** may be roughly vertical, as described by Dr. Chlumsky, but if one will compare the position of the tongue for **l** in *wealth, health*, the manner of taking the position of the tongue is not as described by Dr. Chlumsky for English but as described by him for French. In fact there is no one distinctive manner of taking the position for a front consonant in English (see *Mechanism and Position*, Modern Philology, 1922).

[6]J. L. Barker, published in *Modern Philology*, Nov., 1922.

[7]From 1908–1913 I was acquainted with all of the Abbé Rousselot's students in Paris.

[8]Abbé Rousselot, *Précis de Prononciation Francaise*, p. 58. Also see this article, footnote, p. 164

and in the correction of faulty accent. Parmenter and Treviño state: Barker "found that by interrupting the breath stream between a vowel and a final consonant he was able to pronounce the final consonant as an initial consonant and thereby satisfy the French ear." In their article they do not question this statement.

In their refutation of the Barker theories, they offer kymographic and oscillographic experiments of their own. In regard to my experiments, they state that I disregard the evidence of "uninterrupted voice vibrations throughout the transitions." Other than this, they make no attempt to show that my experiments were either not correctly performed, accurately described, or properly interpreted.

According to them, the kymograph "offers valid indirect evidence regarding the behavior of the breath stream. If the voice vibrations are found to be continuous and uninterrupted from the beginning to the end of a word such as ***bombe***, it follows that there has been no interruption in the breath stream because the vocal cords, which are actuated by the breath, would cease to vibrate if the breath stream were interrupted. Mr. Barker's kymograms frequently show uninterrupted voice vibrations throughout the transitions, but he rejects this evidence and maintains that the kymograph tambour, on account of its mass, continues to vibrate after the impulse from the vocal cords has ceased."[9]

It is true, I have maintained that the inscribing pen "continues to vibrate after the impulse from the vocal cords has ceased." How can it be otherwise? It is generally recognized that any object in movement continues to persist in that movement because of its mass. In the case of the kymograph, to the mass of the tambour must be added the mass of the lever and of the inscribing pen on the end of the lever. (Manufacturers recognize this difficulty of mass and try to reduce it to a minimum by constructing the lever and pen of some light material.) Professor Parmenter and Dr. Treviño assume that the mass of the tambour, of the lever, and of the inscribing pen is of no effect, since they conclude that the instant the vocal cords (actuated by the breath stream) cease to vibrate, the pen ceases to vibrate. If this assumption be true, then Parmenter and Treviño succeeded in overcoming the effect of mass, though they do not claim to have done away with the mass of the tambour, lever, and pen themselves. Is this not absurd?

The inscribing pen does not cease to vibrate instantaneously with the cessation of the production of sound. The effect of mass of the vibrating parts of the kymograph, the tambour, the lever and the inscribing pen is shown in an experiment published in PMLA in December, 1934, Figure 20, page 1181. Any breaks in speech are bridged over in the kymograph owing to the fact that, because of the mass of the moving parts of the apparatus, the vibrating movement of the inscribing pen continues after the exciting cause has

[9]C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French*, PMLA, June, 1936, page 594.

ceased to operate. Unless they have done away with mass in the moving parts of the kymograph or with the persistence of movement due to mass, their conclusion based on the continuity of vibrations appears unfounded.

They assume "that as long as the vocal cords continue to vibrate there is no interruption of the breath stream. . . . to test the kymograph, simultaneous records were made on the oscillograph from a microphone and on the kymograph from a larynx capsule. The subject pronounced the vowel [u:] three times in succession. . . . In every case the kymogram had the same number of vibrations as the corresponding oscillogram. In no case did the kymograph tambour show any tendency to continue vibrating after the impulse ceased as reported by Mr. Barker."[10]

Unless Parmenter and Treviño succeeded in this experiment in overcoming the effect of mass in the kymograph, what they may well have done is to show, not that the kymograph has no "carry over," but that their oscillograph picked up room reverberation or showed other types of "carry over" itself. Their comparative experiment seems to indicate that their oscillographic experiments are unreliable.

"To test the oscillograph, an oscillator of approximately 300 cycles was connected to two of the vibrators. The current to one of the vibrators was interrupted 120 times per second. The resulting oscillogram showed that the vibrator stops in about two ten-thousandths of a second and starts in approximately the same time. This proves that the vibrator does not continue to vibrate after the impulse from the vocal cords has ceased and establishes the validity of the oscillograms for the problem under consideration."[11]

Since their oscillograph vibrated two ten-thousandths of a second after the impulse from the vocal cords had ceased, it evidently would be incapable of registering a "break" of less duration, and it could only serve to register a "break" of longer duration in case the "break" was sufficiently longer to be legible in the oscillogram. The length of such "breaks" (if they exist in accordance with the Barker theories) is unknown. It should also be noted that the Barker theories require a cessation in the effort to expel the breath and the consequent production of sound, but make no statement as to the length of this cessation of effort or of the persistence of sound at a low level in the air. If the cessation of sound is long enough, their oscillograph would reveal it; if in a given case, the "break" is no longer or but little longer than two ten-thousandths of a second, their oscillograph would be useless as a means of testing the truth or falsity of the Barker theories.

The validity of their conclusions is also subject to another reservation. Their conclusions are not valid if the oscillograph recorded vibrations not produced directly by the organs of speech,

---

[10]C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French*, PMLA, June, 1936, p. 595.

[11]*Ibid.*, page 595.

i. e., if it recorded extraneous vibrations, such as room reverberation, amplifier hum, etc.

That this may be the case in the experiments of Professor Parmenter and Doctor Treviño may be seen by an examination of their oscillogram *abdiquer*, Figure 4, opposite page 596, PMLA, June, 1936, reproduced in this article as Figure 1.[12] During the position of closure for the **k**, the tongue shuts off all of the air; moreover, since **k** is a voiceless consonant, no vibrations are produced by the vocal cords. During the time of the occlusion of the **k**, the vibrations of small amplitude here present should be entirely lacking and the occlusion should be represented by a straight line.

"For those (transitions) involving unvoiced consonants, the evidence of the oscillograph is not conclusive because in a filtered oscillogram, an unvoiced consonant is represented by a straight line."[13] Since the occlusion of the **k** is not represented in *abdiquer* by a straight line, this oscillogram was not filtered. Were the other oscillograms filtered? Would unfiltered vibrations bridge "breaks" of any duration in the other oscillograms? Professor Parmenter and Doctor Treviño do not seem justified in offering these oscillograms as evidence that the expulsion of breath is continuous, since the duration of the vibrations in the oscillogram does not coincide with the duration of the production of sound by the speaker.

The oscillograms of Figure 2 to 5 inclusive were made for me some time ago on the occasion of a visit to the Bell Telephone Laboratories. The speakers were three members, American, French, and German, respectively, of the technical staff of the Laboratories who kindly assisted by speaking the word in the ordinary manner. The oscillograms were made in a highly absorbent room with the microphone 3″ to 4″ away from the speaker's lips; the oscillograph was equalized for frequency response up to 9000 cycles and for phase up to 5000 cycles; room reverberations and reflected waves picked up by the microphone

FIGURE 1

Figure 1 reproduces Figure 4 of "The Breath Stream during Transitions in French" by Professor Parmenter and Dr. Treviño, PMLA, June, 1936, page 596. It will be observed that in that part of the experiment in which the vibrations have the smallest amplitude, the position of closure for **k** was being maintained.

[12]Figures 1 and 7 of this article are reproduced with the permission of Doctor Treviño (in the absence of Professor Parmenter); for this courtesy I desire to express my thanks.

[13]C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French*, PMLA, June, 1936, p. 596.

Treviño, PMLA, June, 1936, page 596. It will be observed that in that part of the experiment in which the vibrations have the smallest amplitude, the position of closure for **k** was being maintained.

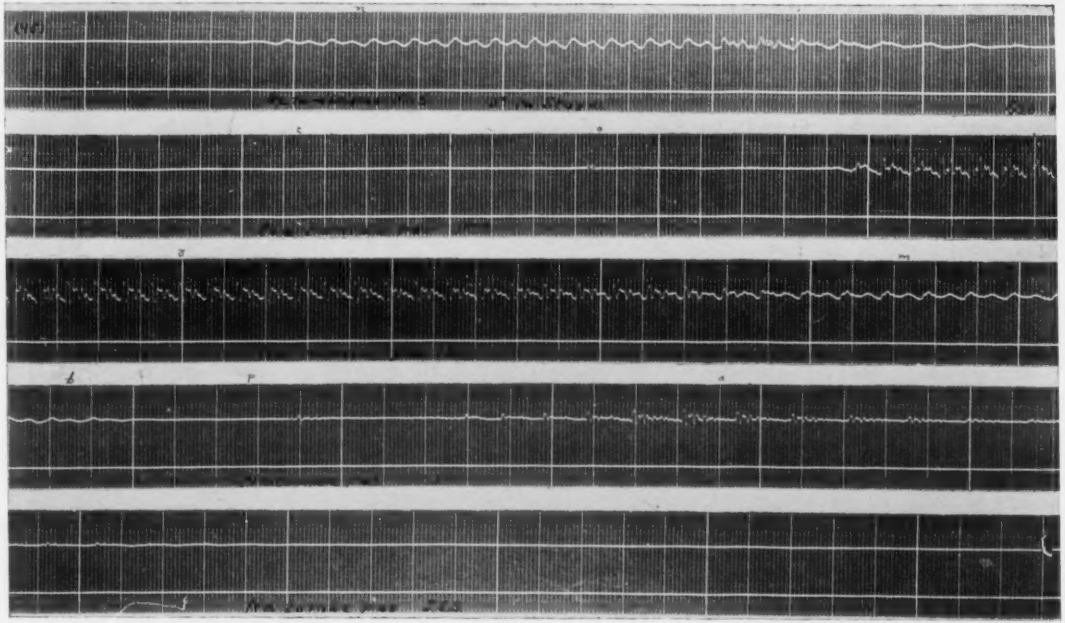


FIGURE 2

Figure 2, *ne tombe pas*, reproduces the American pronunciation of Dr. J. C. Steinberg. **t** is written above the occlusion of the **t** and **o** is written above the small vibrations of the explosion of the **t**. Between the vibrations of the explosion of the **t** and the beginning of the large vibrations of the **õ**, the vibrations continue without interruption. **p** is placed over the line just before the moment of the explosion of the **p**. From the moment of the explosion of the **p**, the vibrations are seen to be continuous throughout the duration of the word.

were not more than one one-hundredth of the amplitude of the direct sound waves which they portray. For the courtesy of performing these experiments I wish to express my gratitude to the Bell Telephone Laboratories, but the conclusions drawn from them are my own and the Laboratories are in no way responsible for them.

Figure 2 reproduces the American pronunciation of "S." It will be observed that the explosion of the **t** and the nasal vowel (*tombe*) merge together according to the Barker theory of continuous effort to expel the breath within (most) syllables in English. From the moment of the explosion of the **t,** the vibrations are seen to be continuous throughout the duration of the word. Theoretically, this continuity of the vibrations could be due to reverberations. However, the set-up was unchanged in the following French and German experiments in which the "breaks" in the continuity of the vibrations are in conformity with the Barker Theories.

Figure 3 represents the French pronunciation of "D" of (French) *ton.* Figure 4 represents the German pronunciation of (French) *ton* by "W." The vibrations of the explosion of the **t** in Figures 3 and 4 die out before the nasal vowel begins. Compare these experiments with the American pronunciation of (French) *tombe,* Figure 2, of "S," where the vibrations are continuous.

All three experiments were performed under the same conditions. Since an interruption in the effort to produce the sounds may be bridged over by reverberations, the absence of such a break in the vibrations between two sounds would not prove that the effort to expel the breath or the production of sound is continuous; on the other hand, because the microphone picks up sounds (of sufficient intensity) from any source, including the vibrations produced by the explosion of the **t** and the vocal cords for **õ** (**on**), the "break" in the vibrations offers sound evidence in favor of the discontinuity of the effort to expel the breath and the production of sounds in French and German in accordance with the Barker theories.

Figure 5 represents the pronunciation of M. DeCoutouly of *acte.* It will be observed that there is a separate explosion both for **k** and **t,** in accordance with the Barker theory that each sound in French is pronounced by itself.

Figure 6 represents the pronunciation of Herr Walther of *akt.* It is evident that the **k** has no explosion. The **t,** however, is exploded. This is in agreement with the Barker theory of German pronunciation: a consonant preceded by a vowel in the same syllable is pronounced as the glide is made on the vowel into the consonant, and a following consonant is exploded, being pronounced by itself.

Other experiments with the oscillograph in confirmation of the Barker theories are available. On the other hand the oscillographic evidence offered in opposition to the Barker theories by Parmenter and Treviño is apparently defective.

The oscillograph does not register the breath stream directly but picks up sound vibrations (from any source) in front of the microphone. Consequently, it is difficult to determine whether the

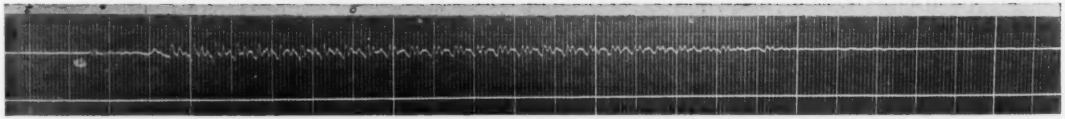


FIGURE 3

Figure 3, *ton*, offers the French pronunciation of Mr. G. C. DeCoutouly. The legend is written at the top: **t,** above the occlusion of the **t; o,** above the explosion of the **t;** and **õ,** above the nasal vowel. Observe that the vibrations of the explosion of the **t** die out before the vibrations for **o** begin. Compare this with the American pronunciation of Dr. J. C. Steinberg of *ne tombe pas*, Figure **2,** where the vibrations are continuous.

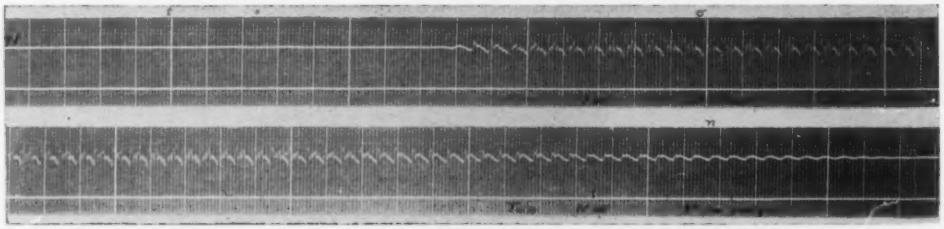


FIGURE 4

Figure 4 represents the German pronunciation of *ton* by Mr. H. Walther. Again the legend is written at the top of the experiment; **t,** above the occlusion of the **t; o,** above the explosion of the **t;** and **õ,** above the nasal vowel. Observe that the vibrations for **õ** begin after the vibrations of the explosion of the **t** have ceased. Compare this experiment with the American pronunciation of *ne tombe pas*, Figure 2, of Dr. J. C. Steinberg, where the vibrations are continuous.

"evidence" offered by it really corresponds to the objective facts of pronunciation.

Evidence in favor of the continuity of the breath stream and evidence contradictory to the theory of continuity are in both cases indirect evidence; however, the two are not of equal value. Since the microphone picks up vibrations in front of it from any source whatsoever, it is to be assumed, if no vibrations were picked up, that there was no source of vibrations present of sufficient intensity to be registered by the oscillograph, including that which would have been furnished by the breath stream if it had been continuous. If vibrations are picked up, it only proves that there were vibrations in front of the microphone but leaves their source a matter for investigation. And there always remain three possible sources of these vibrations other than the one that would arise from the continuity of the breath stream, namely, reverberations from the walls of the "sound-proof" room, which never absorb absolutely all of the sound, reverberations from objects and persons in the room itself, and, in addition, vibrations resulting from some vibration mode of the recording apparatus. Consequently, though the oscillographic evidence reproduced in this article in favor of the Barker theories must be accorded serious consideration, any evidence contrary to them furnished by the oscillograph should be supported by evidence from other sources.

Professor Parmenter and Dr. Treviño offer evidence other than oscillographic experiments. They present experiments with the kymograph. We have seen that they conclude that the kymograph has no "carry over." They could have offered a direct test of the "carry over" of the kymograph, as was done in one of my articles,[14] by producing explosions (pistol shots) of varying loudness and recording the length of time during which the tambour with its inscribing lever and pen continued to vibrate; they could have performed this experiment or a similar one and shown wherein my conclusions are not valid.

Let us examine their kymographic experiments. The larynx tracing in Figure 5, *cette tante*, "shows that the voice vibrations are continuous from the release of the preceding [**s**] until the closure for the [**t**] is complete."[15] This conclusion rests solely on the accuracy with which Professor Parmenter and Dr. Treviño have been able to fix the limits of the sounds.[16] In this they have manifestly failed, because, were the statement true, the **s** would not be **s** but **z**; and the **t** would not be **t**, but **d**. However, the **s** is an **s**, and the **t** is a **t**, for during the occlusion in each case no vibrations are present.

---

[14]J. L. Barker, *Beginning-Consonants and Breath-Control*, PMLA, December, 1934, page 1181.

[15]C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French*, PMLA, June, 1936, page 597.

[16]Others have thought that the division of the sounds as recorded by the kymograph must be made arbitrarily (not accurately). See *La Question du Passage des Sons*, Jos. Chlumsky, Revue de Phonétique, vol. 2, page 80.

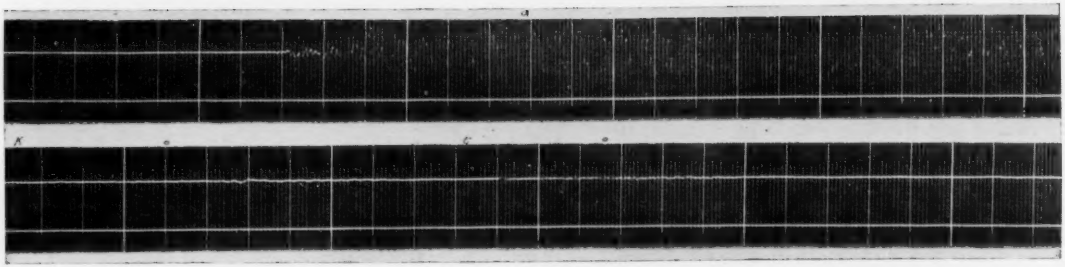


FIGURE 5

Figure 5, *acte*, French pronunciation of Mr. G. C. DeCoutouly. The legend is written at the top of the experiment: **a** is above the vibrations for the **a**; **k**, above the occlusion of the **k**; **o**, above the explosion of the **k**; **t**, above the occlusion of the **t**; and **o**, above the explosion of the **t**. Observe that the separate explosions for **k** and **t** are clearly visible. Compare this experiment with the German pronunciation of Dr. Walther of *akt*, Figure 6, where the explosion of the **k** is completely lacking.

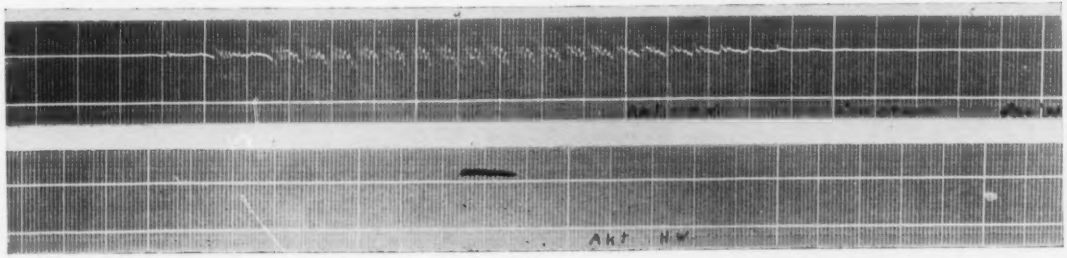


FIGURE 6

Figure 6, *akt*, German pronunciation of Mr. H. Walther. The legend at the top of the experiment is as for Figure 5; however, **k** has no explosion. In the oscillogram for English *act* (no figure), neither **k** nor **t** have an explosion .Figures 5 and 6 are in accord with the Barker theories for German and French pronunciation as contrasted with each other and with the pronunciation of English.

Let us look further at their kymographic experiments. Referring to their Figure 5, *cette tante*, reproduced here as Figure 7, they state: "The spirograph tracing shows that our subject did not explode the first [t] and that he did not lower his tongue between the two [t]'s since the spirograph line would have dropped if he had done so. He, therefore, did not interrupt the breath stream, and the result was a long [t:] with continuous and uninterrupted pressure."[17] If the pressure were continuous and uninterrupted, one would expect a rise in the spirograph line recording the pressure or at least that this line would not drop. If my interpretation of their statement: "The spirograph line would have dropped if he had done so" (exploded the first **t** and lowered his tongue between the two **t**'s) is correct, then they maintain that the air pressure did not decrease and that the line did not drop. An observation of their Figure 5 (our Figure 7) shows that this is not borne out by the experiment. The line after recording the maximum pressure first drops, and then rises again.

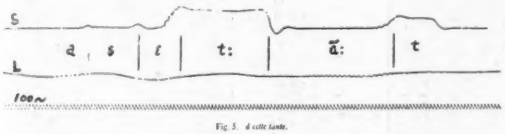


FIG. 7. This reproduces Fig. 5 of Professor Parmenter's and Doctor Trevino's article.

The behavior of this curve would not be different if the Barker theory is true. The first **t** would be exploded with just enough lowering of the tongue to permit the escape of air. Though this would reduce the pressure, it would not reduce it greatly, and consequently, the line would not drop very much. The tongue would then be raised for the plosion of the second **t** and this would increase the pressure again but would not increase it greatly. The important fact is, that between the **t** of *cette* and the **t** of *tante*, the pressure dropped. They state: "The spirograph line would have dropped" if the subject had exploded the first **t**. Consequently, they *should* have concluded that their subject did explode the first **t** in conformity with the Barker theory. Such evidence as is offered by the experiment, their Figure 5 (our Figure 7), does not invalidate but, on the contrary, supports the Barker theory.

They were under no necessity of trying to fix the exact limits of the vowel and the consonant in *cette tante* and of studying the air pressure in the line of the spirograph in order to determine whether the tongue is lowered or not between the **t** of *cette* and the first **t** of

[17] C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French*, PMLA, June, 1936, page 597.

tante.[18] They could have registered the movement of the tongue itself, as I have done in Figure 8. Contrary to the theory of M. Maurice Grammont supported by Parmenter and Treviño of "long" (double) consonants in French requiring that the **t** of *cette* should have no explosion and the first **t** of *tante*, no implosion, it is seen in Figure 8 that each **t** has its separate implosion, plosion, and explosion. And contrary to M. Grammont's theory (also supported by Parmenter and Treviño) that the only difference between a simple (single) consonant and a "long" (double) consonant is in the length of time the position of closure is held, it is seen that the length of the final **t** of *tante* is as great as the combined length of **t** of *cette* and the initial **t** of *tante*.[19] Were the theory of "long" (double) consonants correct, the final **t** of *tante* would be a "long" (double) consonant.

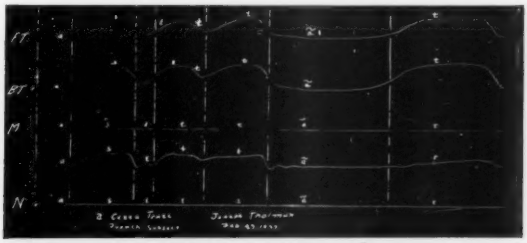


FIG. 8. In the experiment, *à cette tante* by Joseph Thalman, FT, gives the movement of the front part of the tongue; BT, the movement of the back part of the tongue; M, the line of the mouth; S, the line of the spirograph recording the air pressure in the mouth back of the point of contact of the tongue; and N, the line of the nose.

According to this theory, a so-called "double consonant" is not a "double" consonant at all, but a "long" consonant, that is, a consonant differing from an ordinary consonant only in respect to the length of time the position required for the consonant is held. I have shown in M. L. A. meetings and also in a published article[20]

[18] "In order to study the behavior of the breath stream in the transitions preceding the following unvoiced occlusives, the spirograph and larynx capsule were used. The spirograph tracing, marked S on the kymograms, shows the implosion, plosion, and explosion of the occlusive. The larynx tracing, L, shows the voice vibrations. By comparing the two tracings in respect to time, it is possible to determine at what point in the course of the articulation the voice vibrations begin and end."—C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French*, PMLA, June, 1936, page 597.

[19] Numerous experiments are reproduced in my *An Explanation of Differences in the Length and Voicing of Consonants in French and English*, Modern Philology, Feb., 1929; see also *Syllable and Word Division in French and English*, Modern Philology, Feb., 1922.

[20] *Differences in Length and Voicing of Consonants in French and English*, Modern Philology, Feb., 1929.

that a single consonant may be and often is longer than a (so-called) "long" consonant.

This may be seen in Figure 9, representing the pronunciation of *á cette tante* by Mme Janine Cleyet Jacobs and of *that time* by Mr. Randall Barker. By measuring the length of the tongue curves for the two **t**'s of *that time* and the initial **t** of *tante*, it will be seen that the time of the two **t**'s ("long" **t**) is considerably shorter than the time of the one (initial) **t** of *tante*. Such an experiment, which anyone may repeat, should be fatal to a theory, resting on no other evidence and requiring that a "long" (double) consonant be long and a simple consonant short and maintaining that there is no other difference between the two. In fact, the "long" (double) consonant is often short and the simple consonant is often long. This "long" consonant theory can be maintained only with an utter disregard for facts.

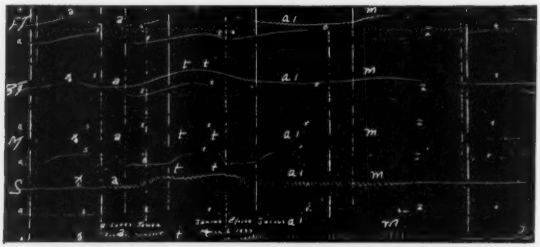


FIG. 9 represents the pronunciation of *that time* by Mr. Randall Barker and the pronunciation of *à cette tante* by Mme. Janine Cleyet Jacobs. In this form the experiment is an accident, but it has been reproduced because it facilitates the comparison of the consonants. The photographer photographed the pronunciation of Mr. Barker and then, forgetting to change the film, photographed the pronunciation of Mrs. Jacobs on the same negative. The line FT gives the movement of the front tongue; BT, the movement of the back tongue; M, the line of the mouth; and S, the line of the spirograph in the pronunciation of Mr. Barker. Just below each of the lines marked FT, BT, M, S, are corresponding lines beginning with *a*, which figure the movement of the front tongue, back tongue, the mouth line, and the spirograph line, respectively, in the pronunciation of Mrs. Jacobs.

The theory of continuity in French pronunciation for which no one has ever offered any serious evidence, probably led M. M. Grammont to the theory of long consonants: "Lorsqu'une consonne finale qui se prononce se trouve devant une consonne initiale, il peut se présenter plusieurs cas suivant la nature de l'une et de l'autre consonne:

1° les deux consonnes sont la même consonne. Dans ce cas on prononce non pas deux consonnes, mais une consonne double, c'est-à-dire une implosion, une tenue et une explosion (theory of "long" consonants defended by Parmenter and Treviño); la consonne finale n'a pas d'explosion, la consonne initiale n'a pas d'implosion

(also defended by Parmenter and Treviño)." M. Grammont cites as examples *pat(te) tordue* and *coup(e) pleine*. As the tongue behaves in *cette tante* or *pat(te) tordue*, so should the lips, according to M. Grammont, behave in *coup(e) pleine*. According to this theory and the theory of "long" consonants, defended by Parmenter and Treviño, as just quoted,[21] the lips take the position of closure for the first **p** and do not open again until the second **p** is exploded, the **p** of *coupe* should have no explosion and the **p** of *pleine* no implosion. Since **p** is a lip consonant, this is easily determined by high-speed photography.[22] Professor Parmenter was present (if my memory serves me well) when the pronunciation of *coupe pleine* by Mme. Moussu of the Alliance Francaise was photographed in the Pathe studios near Paris. Mme. Moussu accepted the statement of the theory by M. Grammont, but her pronunciation of *coupe pleine*, enlarged and shown here as Figure 10, shows the explosion of the first **p**. This high speed photography shows the movement of the lips without the necessity of recourse to the very indirect means employed by Professor Parmenter and Dr. Treviño.

"Figure 6 illustrates the method of passing from an unvoiced occlusive to a voiced occlusive in *lac d'or*. Of this combination Mr. Barker says: 'In French, two such consonants are pronounced successively, and each of the consonants has its (physiological but not acoustic) implosion, its stop and explosion.'[23] The spirograph tracing shows that our subject started the voicing for the [**d**] before the air pressure had reached its lowest point and that there was consequently no interruption of the breath pressure while the position for the [**d**] was being assumed."[24]

The mere fact that the vibrations for the **d** begin before the air pressure as registered by the spirograph falls to zero would seem

---

[21]Maurice Grammont, Traité Pratique de Pronunciation Francaise, pages 95–97.

[22]"It is, of course, impossible to determine from motion pictures whether the breath stream is interrupted or not."—C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French*, PMLA, June, 1936, p. 594.

On page 597, ibid., Parmenter and Treviño seek to prove that the position of the tongue for **t** in *cet(te)* is not left before pronouncing the initial **t** of *tante*, and then add: "He therefore did not interrupt the breath stream." They reason that if the position of the first consonant had been eft and the position of the second consonant taken again, the breath stream would have been interrupted, and on the contrary, if the position for the first consonant is not left before the second consonant is pronounced, that the breath stream is not interrupted; that is, they seek to determine the behavior of the breath stream by noting the behavior of the tongue while pronouncing the two consonants. If this reasoning is correct, then their statement quoted concerning motion pictures is incorrect, since the behavior of the lips in pronouncing a "long" (double) consonant is easily determined by high-speed photography, and having determined what they consider the essential fact, their reasoning would then apply: "He therefore did (or did not) interrupt the breath stream."

[23]J. L. Barker, *An Explanation of the Difference in Length and Voicing of Consonants in French and English*, Modern Philology, Feb., 1929, page 342.

[24]C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French*, PMLA, June, 1936, page 597.

to prove nothing at all, because the air pressure transmitted through the tube and tambour and registered on the kymograph would have a certain retardation as compared with the effort to pronounce. In this case this is especially true, since the tongue raised in preparation for **d** would block in part the escape of the air after the pronunciation of **k**. Professor Parmenter and Dr. Treviño are assuming without proof that the instant the **c** [**k**] was exploded, the air pressure would register as zero pressure.

"Figure 7 shows the words *la place*. Mr. Barker says: '. . . there must be a break in the air current after [**p**] and before [**l**] in French *place*. . . .'[25] As may be seen in the figure, the vibrations for the [**l**] begin before the spirograph line reached its axis. Had the breath stream been interrupted, the spirograph line would have dropped below the axis before the vibrations for the [**l**] began."[26]

No proof is offered for the statement that "had the breath stream been interrupted, the spirograph line would have *dropped below the axis* before the vibrations for the [**l**] began." In fact, were it not for the mass of the vibrating parts of the kymograph, the spirograph line would not drop below the axis. The duration of the spirograph line for **p** need not correspond in length to the duration of the effort to expel the breath when **p** is exploded. The relatively close position of the jaws while pronouncing **p** followed by **l** and the blocking action of the tongue as **l** is prepared cause the breath to escape slowly after the explosion of the **p**, and it is only as the breath escapes that the pressure as recorded by the spirograph line diminishes. It is difficult to say how this experiment can be offered as proof of anything.

"Figure 8 represents the phrase *ne tombe pas*. Mr. Barker finds that in words like *tombe* and *maintenant* '. . . there is a break or interruption in the air current between the (nasal) vowel and the following consonant.'[27] Figure 8 shows that the voice vibrations on the larynx line are continuous from the beginning of [ɔ̃] to the end of [**b**] in the word *tombe* and that there was no interruption of the air current between them."[28] The argument here depends entirely on the assumption that Parmenter and Treviño overcame the effect of mass or on their success in overcoming the operation of what is generally considered an indisputable law of physics.

". . . Figure 9, *maintenant*, shows continuous vibrations on the larynx line from the beginning of the [**m**] to the plosion of the [**t**]. The spirograph line shows vibration throughout the implosion of the [**t**]. This means that there was breath pressure and vibration of the vocal cords during the time of the physiological shift from the

---

[25] J. L. Barker, *Beginning-Consonants and Breath Control in French and English*, PMLA, Dec., 1934, page 1169.

[26] C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French*, PMLA, June, 1936, page 600.

[27] J. L. Barker, *Beginning-Consonants and Breath-Control in French and English*, PMLA, Dec., 1934, page 1167.

[28] C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French*, PMLA, June, 1936, page 600.

position of the [ɛ̃] to that of the [t]."[29] If it is true that the vibrations of the larynx were continuous from the beginning of the **m** to the plosion of the **t,** then the subject did not pronounce **t** but **d,** since the difference in voice is the one essential difference between the two consonants. However, the lack of vibrations during the holding of the position of the **t** shows that the subject really did pronounce **t;** i. e., it shows that the vibrations "on the larynx line" either do not correspond rigidly with the vibrations of the larynx

Fig. 10. (c)oup(e) pl(eine) by Mme. Moussu is reproduced from a high-speed motion picture film taken in Paris in 1925. This illustration begins with **u:** in the pronunciation of *coupe* and ends with the **l** in *pleine.* It may be noticed that the lips close for the first **p,** open slightly for the explosion, close again for the second **p** and open for its explosion. The tongue then moves into position for **l.** Owing to the length of the film it is impossible to produce the entire experiment, but by selecting every fifth print (1, 6, 11, 21, 26, 31, 36, 41, 44), except in the case of **l,** where it was necessary to select the 44th print, because the shift was made so quickly, conclusive evidence is offered that the **p** in *coupe* and the **p** of *pleine* are not pronounced as a long **p,** but as two separate consonants with an explosion following each. (The entire print is in the hands of the editor.)

itself or else the statement that they are continuous on the larynx line from the beginning of **m** to the plosion of **t** is not correct. Moreover, the statement, "the spirograph line shows vibrations throughout the implosion of the **t,**" is also proof of the effect of mass in the kymograph, because had the larynx vibrated until the instant the tongue touched for **t,** not **t** but **d** would have been pronounced. It would seem then that no weight should be attached to conclusions based on this experiment.

---

[29]C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French,* PMLA, June, 1936, page 601.

"Figure 10 shows *le pain*. In the word *pain* Mr. Barker finds that '. . . the [**p**] is exploded, the flow of the air current interrupted, and then when the rest of the shift in position to the vowel has been completed, [ɛ̃] is produced as the expulsion of the air is resumed.'[30] The larynx line in Figure 10 shows that the voice vibrations start at the beginning of the explosion of the [**p**] and continue to the end of the [ɛ̃]."[31] The statement is made "that the voice vibrations start at the beginning of the explosion of the **p.**" A close examination of the figure will show that they start soon after the explosion of the **p.** In fact, if they started at the explosion of the **p,** the **p** would not be a **p** but a **b.** I have already published numerous experiments covering the points above. Professor Parmenter and Dr. Treviño, aside from stating that I neglect continuity of vibrations, apparently did not consider my experiments of sufficient importance to merit examination or refutation. Their own experiments should be examined carefully. If, as I have stated, their descriptions are either faulty (oscillographic experiments) or are not correctly described or the description of the experiments does not justify the conclusions drawn, no weight should be attached to their conclusions.

"Figures 1 to 4 are oscillograms of the words *même, belle, bombe,* and *abdiquer.* It will be seen that the voice vibrations are continuous from the beginning to the end of the first three words. This indicates that there was no interruption of the breath stream during the transitions."[32]

The argument in connection with Figures 1 to 4[33] "that the voice vibrations are continuous" and that therefore "there was no interruption of the breath-stream during the transitions" is the usual argument and the only factual "evidence" that has ever been advanced in favor of the theory of the continuous expulsion of the breath.[34] Unless it is assumed that when a pendulum is hit, it ceases to swing with the termination of the impact of the blow and that mass is of no effect, I do not know how anyone can consider this position tenable; nevertheless, it is the argument of Parmenter and Treviño, and has been widely accepted without any other supporting evidence.[35]

---

[30]J. L. Barker, *Beginning-Consonants and Breath-Control in French and English,* PMLA, Feb., 1934, page 1177.

[31]C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French,* PMLA, June, 1936, page 601.

[32]C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French,* PMLA, June, 1936, page 596.

[33]C. E. Parmenter and S. N. Treviño, *The Breath Stream during Transitions in French,* PMLA, June, 1936, page 596.

[34]Experimentelle Phonetik, Panconcelli-Calzia.

[35]Shortly before the publication of Parmenter's and Treviño's article, Dr. Hubert Pernot wrote under *Transcriptions Phonétiques* in the *Revue de Phonétique*:

"Les deux textes qui suivent donnent des exercices destinés aux commeçants. Non seulement la prononciation y est syllabique, mais chaque syllabe doit etre prononcée séparément. On obligera les élèvès à lire très lentement, en s'arrêtant

entre les syllabes. Quand on jugera que leurs consonnes et surtout leurs voyelles sont suffisamment bonnes, on pourra leur faire grouper les syllabes en mots, mais en les obligeant toujours à prononcer lentement. Les principes sur lesquels est basée la division en syllabes seront donnés plus tard dans cette Revue.

"On remarquera que le mot *cigale* par exemple est considéré comme formant trois syllabes (**si 'ga- lə**), chacune de ces syllabes étant du type *consonne-voyelle*. Le petit (ə) qui termine certains mots est une résonance vocalique plutôt qu'un (ə) véritable. Nous accoutumons nos élèves a prononcer ainsi au début et pendant assez longtemps, pour leur apprendre la détente finale, qui est un fait important de la prononciation française. L'usage de cet (ə) aide en outre à améliorer L'articulation de certaines consonnes, comme l' (ə) final américain dans le mot *bel* (**b ɛ-lə**)."—*Revue de Phonétique*, 1928, p. 147.

Observe that words are to be broken up into syllables of the type "consonant-vowel" and especially that final consonants are to be pronounced with a following explosion (this term mine) which is a "vocalic-resonance" rather than a true mute **e** (ə) (distinction used by me), and observe also that this applies to words of the type *bel* (used in my first experiment) where the word ends in a consonant.

We are told that this is "an important fact in French pronunciation" ("un fait important de la pronunciation francaise") and that the principles on which it is based will be given later in the *Revue de Phonetique*.

This seems to imply that this syllabic division rests on some discovery to be made known by Dr. H. Pernot. Dr. Pernot saw my film (first shown at the meeting of the MLA in Chicago, 1925) when it was presented to the members of the Société de Linguistique de Paris in 1927, and he heard me discuss it in lectures and in private conversations. The indication of syllabic division in the *Revue de Phonétique* appeared for the first time in 1928, twelve years after I had indicated the "detaching" of consonants in *Modern Philology* and three years after I had used it in a text. Dr. Pernot will in all probability not publish the promised "principes sur lesquels est basée la division en syllabes," since in a letter he says: "je ne pense pas qu'il y ait découverte, ni de votre part, ni de la mienne. Elle est pratiquée dans nos écoles primaires depuis des siècles." In any case such a syllabic division is not enough. In the first place it is inconsistent. There is no syllable division as such in a one-syllable word of the type *bel*, yet the final l must be detached. Furthermore, there is no syllabic division to be made and Dr. Pernot makes none "dans les mots du type *dormaient* (**dɔr m ɛ**), où l' [**r**] s'allie avec la voyelle précédente." If the **r** is joined on to the preceding vowel, as indicated by Dr. Pernot, the result is one of the various **r** sounds in use in English-speaking countries. In fact, any consonant so pronounced is not a French consonant at all but an English consonant.

Moreover, lengthy articles on "**e** muet" (such as *Les Voyelles Parisiennes l'e muet*, Hubert Pernot, *Revue de Phonétique*, 1929, pp. 64–151) are without practical value if the detaching of French consonants (Barker theory) is ignored, and unnecessary, if students are taught to detach them.

It should also be noted that though there is usually a "final release" after French consonants, this release must be used only after plosive consonants. After **m, n, l, r,** and after all continuants, the one essential is that the transition from preceding vowel to the consonant be made while no air is being expelled; the explosion (as separate from the "tenue" may be and sometimes is entirely lacking.

Even had Dr. Pernot been able to include all cases of French pronunciation in his syllabic ("pratiquée dans nos écoles primaires depuis des siècles") division it still would have been entirely insufficient. It is absolutely necessary to include the cessation in the effort to pronounce, and that, not only between vowel and following consonant in the same syllable, but also between consonant and consonant, and consonant and following vowel. If there is no cessation in the effort to pronounce between consonant and vowel, it is impossible to avoid an audible off-glide of the consonant and an on-glide of the vowel or the dipthongization of the first part of the vowel. If there is no cessation in the effort to pronounce between vowel and following consonant, there must of necessity be an audible glide between the vowel and the following consonant, and the last part of the vowel is diphthongized. Unless the second consonant is prepared before the first is pronounced, so that one is exploded into the other, as **tr** in English *try*, if there is no cessation in the effort to pronounce between consonant and

consonant, a double consonant must of necessity result, as in *tell Lawrence.* (For details see articles cited below.)

Mlle. Nicolette Pernot has applied this "prononciation syllabique" to the teaching of French pronunciation in her "Cours de Phonétique" used at Middlebury College. She says, Feuille 14: "Toutes les fois qu'on s'arrête pour respirer, c'est-à-dire généralement à la fin d'un groupe phonétique, on entend après la consonne finale une *détente,* qui est un petit bruit vocalique accompagnant la remise en position des organes. C'est une sorte de petit e muet, et nous l'indiquons dans l'alphabet phonétique par "**ə**"; Feuille 6: "Placer les organes dans la position requise avant de commencer à prononcer" and "ne pas mouvoir les organes pendant toute la durée de la voyelle"; Fueille 4 et Feuille 5: "Si deux consonnes précedènt une voyelle, ces deux consonnes appartiennent généralement à deux syllabes différentes: quel-con-que, **i-ny-til-mā.**"

The "prononciation syllabique" is not enough. It is true that one hears the "détente" after a "consonne finale." It is also true that the position for a vowel in French should be taken "avant de commencer de prononcer" and that the "organes" should not be moved during the entire duration of the vowel; but what is the use of telling the student to take the position of the vowel before he begins to pronounce and to hold the position of the vowel until after the pronunciation is completed, if conformity to another part of the theory and to the phonetic notation makes this impossible? How can the position for the vowel be taken before beginning to pronounce **ɛ** (e) in "quel-con-que," if contrary to the Barker theory, there is no break in the expulsion of breath between the **k** (qu) and the **ɛ** (e)? A series of vowel sounds will be produced of necessity while the shift in tongue position is being made from the release of the **l** to the position of the **ɛ** (e); that is, the beginning of the **ɛ** (e) will unavoidably be diphthongized, if the expulsion of the breath continues while the position for the consonant is being left and the position for the vowel is being taken. Again, if there is no break in the expulsion of breath between **ɛ** (e) and the following **l**, of necessity the pronunciation will be continuous while the transition is made from the tongue position required for **ɛ** (e) to that required for **l**, and the last part of the vowel will be diphthongized, such diphthongization being absolutely unavoidable unless, as in accordance with the Barker theory, the effort to expel the breath is suspended while the shift in position is being made from **ɛ** (e) to **l**. Consequently, the recommendation not to move the organs during the entire duration of the vowel and the recommendation to pronounce the **l** with the **ɛ** (e) cannot both be obeyed. If either is done the other is impossible.

Mlle. Pernot also accepts the theory of "long" consonants ("consonnes prolongées"), Feuille 22: "Si un mot se termine par une consonne et si le mot suivant commence par la même consonne, ces deux consonnes se prononcent comme une seule, qui serait longue et qui aurait: une implosion, une tenue (longue), et une explosion." Then the pronunciation from vowel to "long" consonant (consonne prolongée) must continue while the vowel position is being left and the consonant position is being taken: the position of the vowel cannot be held without change, and the end of the vowel is necessarily diphthongized; and the pronunciation from "long" consonant ("consonne prolongée") to vowel must continue while the consonant position is being left and the vowel position is being taken: the position of the vowel cannot be taken before beginning to pronounce, and the beginning of the vowel is of necessity diphthongized.

In English, the effort to expel the air from vowel to consonant in the same syllable is continuous and, in the English pronunciation of French words like *tombe, chance, quelconque,* it results in the production of a consonant as the transition is made from the nasal vowel to the following consonant. If the following consonant is **b** or **p**, the "parasitic" consonant is **m;** if the following consonant is **t, d,** or **s,** the "parasitic" consonant is **n;** and if the following consonant is **g** or **k**, the "parasitic" consonant is **ng.** In all cases the production of the "parasitic" consonant is not intended, but it is produced mechanically as part of the glide: it is the off-glide of the nasal vowel and the on-glide of the following consonant.

The theory of continuous effort to expel the breath, not only results in the contrary-to-fact theory of "long" consonants, but it explains nothing and leaves phonetics a mass of unrelated phenomena. Its application to the correction of accents results in failure.

The Barker theories offer an explanation of the existence of diphthongs, obscure vowels, strongly accented vowels, short consonants, and weakly voiced consonants in the Germanic languages, as contrasted with the Romance and Slav languages. They also explain differences in movement, as recorded by the kymograph and by photography,[36] and applied to the correction of accents, result in unquestioned success.

The evidence in opposition to the Barker theories offered by Professor Parmenter and Dr. Treviño appears to lack validity. Either their experiments themselves seem to be defective or the facts revealed by the experiments appear to be incorrectly stated; moreover, aside from evidence depending on the overcoming of the effect of mass in the kymograph, they present no evidence to invalidate any of the many experiments published by me. The oscillographic and kymographic evidence presented in this paper leads to the conclusion that in accordance with the Barker theories as heretofore published, there is a cessation in the effort to expel the breath and in the breath stream in passing from consonant to vowel, from vowel to consonant, and from consonant to consonant within the phonic group in French. The same may be said of the numerous Slavonic, Romance, Oriental, and other languages mentioned in my article published in the March, 1936, issue of this journal, and if this basic habit is not corrected in the American pronunciation of these nationalities they will always speak with a foreign brogue.

---

[36] J. L. Barker, *Syllable and Word Division in French and English*, Modern Philology, Feb., 1922.

J. L. Barker, *The Formation of Voiceless Consonants in French and English*, Modern Philology, Nov., 1922.

J. L. Barker, *Neutral or Supporting Vowels in French and English*, Modern Philology, Feb., 1925.

J. L. Barker, *An Explanation of the Difference in Length and Voicing of Consonants in French and English*, Modern Philology, Feb., 1929.

J. L. Barker, *Rate, Direction, and Continuity of Movement in French and English Speech*, Publication of the Modern Language Association of America, Dec., 1930.

J. L. Barker, *Beginning-Consonants* and *Breath-Control in French and English*, Publication of the Modern Language Association of America, Dec., 1934.

J. L. Barker, *Beginning-Consonants* and *Breath-Control in French and English*, Language, Feb., 1934.

J. L. Barker, *Mechanism and Position*, Modern Philology, Nov., 1922.

PERIODICAL REVIEWS

By CHARLES H. VOELKER
Director, Dartmouth College Speech Clinic

DISABLED READERS AT DARTMOUTH COLLEGE. By ROBERT M. BEAR. *Bulletin of the American Association of Collegiate Registrars*, pp. 138–144 (January) 1937.

At Dartmouth each student is given an Iowa Silent Reading Test, and photographs of eye-movements in reading are made as a part of the physical examination with an Ophthalm-O-Graph. "Aniseikonia, an inequality in the relative size and shape of the ocular images of the two eyes as they are produced in the brain has only recently been measured and recognized as frequently present in cases of reading disability. Pioneer work in this has been done by the Department of Research in Physiological Optics of the Dartmouth Medical School. The ordinary visual examination neither detects the defect nor, because of this failure in cases which have the defect, does it afford the basis for a full refractive correction. At Dartmouth according to test scores, twice as many were low in rate as in comprehension. Research has shown that greater efficiency in rate of reading is a determining factor in the superiority of good readers at least in reading tests. From these facts the suggestion has been made that, in an institution like Dartmouth with a selective system of admissions, improvement in rate is the greatest need of the majority of disabled readers."

THE GENERAL INTELLIGENCE OF COLLEGE STUTTERERS. By MAX D. STEER (Purdue University). *School and Society*, 44: 862-864 (Dec. 26) 1936.

The average IQ for 87 college stutterers was 116.5. 6.8% ranged below 99; 24.1% between 100 and 109; and 68.9% between 110 and 133. The range was found to be from 80 to 139. The median normal was 111, which in the case of stutterers was 118. 80% of the intelligence quotients were at the normal median or above. By percentile, with 50 for normal, the stutterers rate 69.

SOME EXPERIMENTAL CONTRIBUTIONS TO THE PSYCHOLOGY AND PSYCHOPATHOLOGY OF STUTTERERS. By E. INGEBREGTSEN (Speech Teacher at Granhaug Off. Skole, Lysaker, Oslo, Norway). Menasha, Wis.: *American Journal of Orthopsychiatry*, 6: 630–650 (October) 1936.

"Summing up we can say that the developed stutterer presents the following characteristic picture of symptoms: 1. Reduced attention. 2. Weakness of will—great suggestibility. 3. Reduced memory of languages—small storage of words. 4. Logical displacement—amnestic confabulation. 5. Motor amusia. 6. Stereotyped perception—tendency to perseveration. 7. Indolence—bluntness of effect. 8. Derangements of motility. 9. Signs of depression. 10. Repressions. 11. Restrictions."

THE RELATION OF BILINGUALISM TO STUTTERING

A Survey in the East Chicago, Indiana, Schools

LEE EDWARD TRAVIS, Ph. D., WENDELL JOHNSON, Ph. D., AND JAYNE SHOVER,
State University of Iowa

East Chicago, with its mills and plants, is an industrial center. Men have come from nearly all the nations of the world, bringing with them their languages and cultures.

"The presence of such a large foreign population and of such a diversity of languages has created a very real problem for the schools. Children not only must learn to speak English, but they must overcome many errors of diction and expression caused by their daily association with foreign languages."[2]

It is one phase of this problem that constitutes the subject of this investigation. It is the particular purpose of the present study to ascertain whether there is any relation between bilingualism and stuttering. So far as the writers are aware, this problem has never been systematically investigated. The importance of investigating it, however, is suggested by a critical evaluation of stuttering phenomena, and has been implied concisely in a statement by O'Shea: "Let any adult attempt to express himself upon any familiar subject in a foreign tongue of which he is not thoroughly a master and he will show some confusion and inhibition as does the child who is just beginning his work in composition."[3]

I. SUBJECTS

This survey was conducted in the public schools of East Chicago, Indiana. The various schools in the system which co-operated were: Riley, Columbus, Franklin, Washington Elementary, Harrison, McKinley, Garfield, Lincoln and Mark. A total of 4,827 children were surveyed individually by Miss Shover; 2,405 of these were boys, and 2,422 were girls. They ranged in age from four to seventeen years with an average age of 8.54 years. The grades

---

[1]From the State University of Iowa Speech Clinic, Professor Lee Edward Travis, Director. All data and materials pertinent to this study are on file in the State University of Iowa Speech Clinic.

Acknowledgments are due to Superintendent Roy W. Feik, the Medical Inspection Department, the Psychiatric Clinic, the Psychometric Division, and the teachers of the East Chicago, Indiana, public schools, who co-operated in making this survey possible.

[2]Edith R. Shannon, Mattie L. Hatcher and Edna McQuire, "Centers of Interest in the Middle Grades," p. 503; unpublished manuscript.

[3]O'Shea, M. V. "Linguistic Development and Education," Macmillan Company, New York, 1907, page 266.

ranged from kindergarten through the sixth grade plus the "opportunity"[4] group.

Of the 4,827 subjects investigated 2,399 spoke English only. In this group there were 665 colored subjects. There were 2,322 who spoke English and one other language. These subjects are classified as bilinguals. The foreign languages spoken by the bilinguals include: Croatian, Polish, Czechoslovakian, Greek, Russian, Hungarian, Jugoslavian, Lithuanian, Roumanian, Italian, Mexican, Ukranian, Bulgarian, Syrian, Armenian, Macedonian, Swedish, Yiddish, Serbian, Austrian, Turkish, Chinese, French, German, Finnish, Danish, and Bohemian.

The languages of the polyglots (those speaking three to five languages, including English) are duplicates of these listed above for the bilinguals. In this group there were 37 boys and 52 girls. New immigrants arrive almost daily in East Chicago: for the most part brothers and sisters who have been living with other relatives in Europe. These boys and girls may arrive with only one foreign language at their command; or if, as in some instances, they have migrated from a bilingual country,[5] they arrive as bilinguals without a command of the English language. The foreign speaking cases who did not speak English totaled 27, ten of which were boys and seventeen of which were girls.

II. Procedure

Miss Shover interviewed each child at his respective school. The questionnaire used in connection with this interview consisted of the following items:

SPEECH DEFECT SURVEY—EAST CHICAGO PUBLIC SCHOOLS

Name.....School.....
Age.....Grade—Section.....
Sex.....Speech Defect.....
Languages spoken: L., L. S., S., F. A., V., O. I., A.[6]
No.....
Kind.....
Proportion of home speech which is English.....
Proportion of school speech which is English.....
Languages spoken by mother.....
Languages spoken by father.....

[4]Group for mentally retarded children.

[5]Due to the political constituency of certain countries, they are bilingual. Russia, for instance, has a state language, Russian; but since Armenia, Ukrania and Georgia are subservient politically to Russia, their languages become secondary but remain active home and church languages. This situation, though characteristically European and Oriental, is transferred bodily by means of immigration to such American communities as East Chicago, Indiana. It should be noted that in East Chicago there are many children who attend schools conducted at their respective churches and in a foreign language during summer vacations or on Saturdays. This assures continuance of bilingualism for some time.

[6]L.—lisping; L. S.—letter substitution; S.—stuttering; F. A.—foreign accent; V.—Voice; O. I.—oral inactivity; A.—aphasia.

The length of time consumed in securing the above data totaled six and one-half weeks. The Medical Inspection Department, the Psychiatric Clinic and the Psychometric Division, all of the East Chicago Public School System, each gave additional reports on the 126 stutterers found among the 4,827 subjects surveyed. In many cases, especially among non-English-speaking subjects, an interpreter was essential in determining whether or not stuttering was present.

Graded readable selections containing the most frequently used sounds[7] were translated into the child's native language, and these selections together with actual conversation formed the basis of the interpreter's deductions. Interpreters were enlisted from the ranks of the following representative walks of life: Personnel directors of oil refineries and steel mills, priests, merchants, foreign schoolmasters, and interpreters for docks and unloading companies. For the most part the subjects seemed to be rather elated over their ability to converse in additional languages, which lightened the task of surveying considerably.

III. Results

Of the 4,827 children investigated, 126 or 2.61 per cent, were stutterers. In Table I are collected the percentages of stuttering for each of the various language groups of the study. This number is somewhat higher than those found in other surveys conducted by Blanton, Francis, Root, and Wallin (see Bibliography).

TABLE I

	English Speaking Only	Bilingual*	Total White	Colored	Three Languages†	Foreign Only
Boys.....	2.72	3.70	3.39	5.77	5.88	10.00
Girls.....	.83	1.90	1.45	1.98	0.00	5.88
Total.....	1.80	2.80	2.43	3.76	2.38	7.41

From Table I it will be seen that for all cases of bilinguals the percentage of stuttering was 2.80 per cent, while for those children who spoke only English it was 1.80 per cent. Statistical analysis reveals that there are 98 chances in 100 that this difference is a true one. If we group together all cases speaking English plus one or more foreign languages, we obtain a per cent of stuttering for this combined group of 2.82. Comparing this group with the group speaking English only, we find that there are 99 chances in 100 that there is a true difference.

---

[7]According to the Detroit Articulation Test Sheet.

*Bilinguals were those who spoke English and one foreign language.

†Those with three languages spoke English and two foreign languages.

For 62 white stutterers I. Q.'s could be obtained. Ten of these were Stanford-Binet I. Q.'s and fifty-two were Pintner Non-Language I. Q.'s. The bilinguals were compared with the white subjects who spoke only English.

For the bilingual stutterers 6 Stanford-Binet I. Q.'s ranged from 55 to 110, with a mean of 84.6. Thirty-six Pintner Non-Language I. Q.'s ranged from 73 to 128, with a mean of 90.3.

For the English-speaking stutterers, four Stanford-Binet I. Q.'s ranged from 99 to 120, with a mean of 112.5. Sixteen Pintner Non-Language I. Q.'s ranged from 79 to 118, with a mean of 93.9.

The age of onset of stuttering in 13 of the boys coincided with the introduction of a second language; for 4 girl stutterers the same was true. These cases constitute 26 per cent of the total group of bilingual stutterers.

Of the bilingual stutterers 66.2 per cent were boys and 33.8 per cent girls. Of the English speaking group 77.4 per cent were boys and 22.6 girls.

Only three cases[8] exhibited a sufficient degree of personality maladjustment to be referred to the Child Guidance Clinic as behavior problems.

Physical examinations on the stutterers were conducted by the Medical Inspection Department of the schools. Forty per cent of the bilinguals and 25.8 per cent of English-speaking subjects showed defective teeth. Diseased and enlarged tonsils were found in 13.8 per cent of the bilinguals and in 6.5 per cent of the English speaking. Rachitic deformities and poor nutrition each had 10.8 per cent in the bilinguals and 12.9 per cent in English speaking cases. Defective vision was present in 9.2 per cent of the bilinguals and in 22.6 per cent of English speaking cases.

The bilinguals have from none to seven siblings; they have from none to two siblings who stutter. The monolinguals have from none to six siblings; having from none to three siblings who stutter. Further delving into the size of family shows a total of 181 siblings in bilinguals' families out of which 65 stutter; in English speaking families, out of 65 siblings 29 stutter.

Of these six factors investigated in addition to bilingualism, only intelligence seems to be of any significance, and the number of cases involved in this comparison is quite small. The evidence does seem to indicate that bilinguals are on the average less intelligent than the English speaking stutterers. In so far as intelligence may be related to stuttering, this factor should be considered in evaluating the significance of bilingualism as such in relation to stuttering.

---

[8]One was a psychopathic inferior, one a constitutional inferior, one a social adjustment case. Analyses given by Dr. Hulbert, Consulting School Psychiatrist for East Chicago Public Schools.

IV. Summary and Conclusion

A survey was conducted on 4,827 school children in East Chicago, Indiana, to determine the influence of bilingualism and other factors upon incidence of stuttering. From data obtained it appears probable that there are significantly more stutterers among the bilinguals than among the children who speak only English; the respective percentages are 2.80 and 1.80, and the chances are about 98 in 100 that the difference is greater than zero. We cannot be certain, however, that this difference is due solely to the factor of bilingualism. It may be due to the economic insecurity and emotional instability found in many foreign homes as a result of the recent economic depression, or it may be due merely to a confusion which arises from being placed in a totally strange and new environment. A check on several factors other than bilingualism indicated that the somewhat lower average I. Q. of the bilinguals should be considered in evaluating the significance of bilingualism as such in relation to stuttering.

That there is a difference between the bilinguals and English speaking subjects may be no more significant than the fact that the difference is really quite small. It is not to be overlooked that 97.20 per cent of the bilinguals do not stutter.

BIBLIOGRAPHY

(1) Blanton, Smiley. A Survey of Speech Defects. J. Educ. Psychol., 7, 1916, 581–592.

(2) Frances, Josephine T. A Survey of Speech Defectives of Iowa City, Iowa. Unpublished Master's Thesis, State University of Iowa, 1930, pp. 39.

(3) Root, A. R. A Survey of Speech Defectives in the Public Elementary Schools of South Dakota. Elem. School J., 26, 1926, 531–541.

(4) Wallin, J. E. W. Speech Defectives in a Large School System. Miami Univ. Bull., Series 25, 1926, pp. 45.

BIBLIOGRAPHY—(*Continued*)

PHONETICS

GEMELLI, A. AND PASTORI, G. *Analyse electrique du langage: I. Recherches sur la natur des vowelles. Archiv neer phonetique exptl.* 1934.

GEMELLI, A. AND PASTORI, G. *La durata minima delle vocali sufficiente alla loro percezione. Archivo di fisiologie,* 1934; 440–452.

VOELKER, CHARLES H., M.A. *The Occurrence of Homophenous Articulation in American Usage.* Annals, May, 1934.

VOELKER, CHARLES H., M.A. *The Usage of Vowel Positions.* Annals, Jan., 1935.

PRESCHOOL DEAF CHILD

Early Training and Parent Responsibility. Home and School. Volta Review, Apr., 1934; 218.

FORSYTH, WINIFRED J. *What the Teacher Can Do for the Pre-school Child.* Volta Review, Oct., 1933; 413.

MILLER, MALINDA K. *The Status of the Preschool Deaf Child.* Annals, Nov., 1934.

MONTAGUE, HARRIET. *The Nursery School and the Deaf Child.* Volta Review, 1935; 207.

Mothers Talk It Over, The (Includes The Bored Baby, The Naughty Baby, Things for the Baby to Do, Antidote for Discouragement). Volta Review, Aug., 1934, 456.

NORDSTROM, MABEL. *One Boy's Progress in Sense Training, Ear Training, Lip Reading and Speech.* Volta Review, Nov., 1934. 659.

NORDSTROM, MABEL. *Billy's Second Year of Progress.* Volta Review, Feb., 1936; 75.

Nursery Schools for the Deaf in Russia (note). Volta Review, Sept., 1933; 390.

RAU, E. F. *Methods of Educating Very Young Deaf Children* (Translated from the Russian and digested by Helen G. Smith). Volta Review, Sept., Oct., Nov., 1935.

Residential Schools and Day Schools (The Mothers Talk It Over). Volta Review, Mar., 1935; 143.

SCYSTER, MARGARET. *Summary of Four Years' Experiment with Preschool Deaf Children at the Illinois School for the Deaf.* Annals, May, 1936.

WAGNER, MARY A. *The Experimental Preschool Group of Clarke School.* Annals, Nov., 1935.

WINSTON, MATIE E. *What the Parent Can Do for the Preschool Child.* Volta Review, Oct., 1933; 411.

WORK, MARGARET. *Is Baby Deaf?* Volta Review, Nov., 1936; 648.

PREVENTION OF DEAFNESS

HEIDER, F. *The Influence of the Epidemic of 1918 on Deafness: a Study of Birth Dates of Pupils Registered in Schools for the Deaf.* Reprint, Amer. Jour. Hygiene, May 1934; 756–762.

LOVE, J. K. *Towards the Prevention of Deaf-Mutism: A review of forty year's progress.* Jour. Laryngol. and Otol., xlvii, 556–560.

LOVE, J. K. *Deafness and Common Sense.* London, Frederick Muller, Ltd.

MONTAGUE, HARRIET. *Pre-natal Medication a Possible Factor of Deafness.* Volta Review, Feb., 1935; 69.

RUNK, R. L. *Simple Rules that May Save Your Hearing.* Life and Health, Nov., 1936; 11, 26, 27.

SHAMBAUGH, GEORGE E., JR. *The Prevention and Treatment of Deafness.* Annals of Otol., Rhinol. and Laryngol., 1934; 513–524.

SHAMBAUGH, GEORGE E., JR. *A Workable Plan for the Cooperation of Physicians and Leagues for the Hard of Hearing in the Scientific Study of Deafness.* Proceed. 16th Ann. Meet. A. S. H. H., 1935; 23–25.

STEVENSON, R. S. *Preventing Deafness.* London State Journal of Med., April, 1935.

TAYLOR, H. MARSHALL. *Pre-natal Medication as a Possible Etiologic Factor of Deafness in the New Born.* Arch. Otolaryngol, Dec., 1934; 790–803.

TIMBERLAKE, JOSEPHINE B. *Helping the Doctors to Study Deafness.* Volta Review, May, 1935; 291.

Washington Makes Progress in the Fight Against Deafness. Volta Review, May, 1935; 305.

PSYCHOLOGY OF DEAFNESS

BERRY, GORDON. *Deafness as a Psychological Entity.* Annals of Surgery, Jan., 1935.

BERRY, GORDON. *Psychology of Progressive Deafness.* Rep. Jour. A. M. A., Nov. 18, 1933.

BRILL, TOBIAS. *Mental Hygiene and the Deaf.* Annals, Sept., 1934.

FENTRESS, T. L. *Are Our Deafened People Hypersensitive?* Rep. Proceed. 14th Ann. Meet. A. S. H. H., 1933.

GUTTMACHER, M. S. *Effects of Handicap on Personality.* Proceed. 15th Ann. Meet. A. S. H. H., 1934; 98–102.

HARKNESS, MARGARET. *Deaf Children Do Have Imagination.* Volta Review, Oct., 1936; 567.

LUMSEN, J. *The Evolution of Modern Psychology with Some Reference to the Deaf.* Teacher of the Deaf, June, 1935.

MILLER, ELEANOR OLMSTEAD. *Studying the Psychology of the Deaf.* Volta Review, Dec., 1935; 738.

MOLITCH, M. *Hearing Defects in Behavior Problems.* Jour. Juv. Research, Jan., 1936; 15–19.

PINTNER, R. *Emotional Stability of the Hard of Hearing.* Jour. Genetic Psychol., 1933; 293, 311.

PRUSIA, CLAUDE W. *Emotional Stimulus.* Volta Review, June, 1934; 357.

SAUNDERS, NIDA. *Personality Problems.* Volta Review, Jan., 1934; 9.

(*To be continued*)

Annual Convention

of the

American Speech Correction Association

Meeting Jointly with the American Academy of Ophthalmology and Otolaryngology

CHICAGO, ILLINOIS

October 11, 12, and 13, 1937

Members planning to attend the convention from points in the territory west of Chicago, St. Louis and the Mississippi River and east of Ogden, Salt Lake City, Utah, Albuquerque, M. N., and El Paso, Texas, can purchase through round-trip first-class tickets to Chicago at the time of your meeting at fares based on 2 cents per mile in each direction. Tickets at these fares are good for transportation in sleeping and parlor cars upon payment of the usual charges for space occupied. Tickets good for transportation in coaches only will be on sale at fares based on approximately 1.8 cents per mile. All classes of tickets bear limit of 30 days from the date of sale.

Those who will travel to your convention from points west of Ogden, Salt Lake City, Utah, Albuquerque, N. M., and El Paso, Texas, may avail themselves of the low summer excursion fares which will be in effect to Chicago at the time of the meeting. First-class summer excursion tickets bear limit of October 31, 1937, for return, and coach and intermediate class summer excursion tickets bear limit of 6 months from the date of sale.

Members should be instructed to consult their local ticket agents for information of the most advantageous arrangements and details of specific fares from starting point. No credentials will be required.

Rates are much as they were last Christmas, but less favorable to the east and slightly more favorable to far west. In general, members may purchase on any day before the convention reduced round trip fares from the South and West at 2 cents per mile each way, good in Pullman cars. We from the East have to pay the full maximum fare of 3 cents per mile each way in Pullmans and 2 cents per mile each way in coaches. Berth extra.